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CONTENTS.

XII. *On double Images caused by atmospherical Refraction. By* William Hyde Wollaston, *M. D. F. R. S.* - p. 239

XIII. *Investigation of the Powers of the prismatic Colours to heat and illuminate Objects; with Remarks, that prove the different Refrangibility of radiant Heat. To which is added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers. By* William Herschel, *LL. D. F. R. S.* - p. 255

XIV. *Experiments on the Refrangibility of the invisible Rays of the Sun. By* William Herschel, *LL. D. F. R. S.* - p. 284

XV. *Experiments on the solar, and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different. By* William Herschel, *LL. D. F. R. S.* p. 293

XVI. *Chemical Experiments on Zoophytes; with some Observations on the component Parts of Membrane. By* Charles Hatchett, *Esq. F. R. S.* - - - p. 327

XVII. *On the Electricity excited by the mere Contact of conducting Substances of different kinds. In a Letter from Mr.* Alexander Volta, *F. R. S. Professor of Natural Philosophy*

in the University of Pavia, to the Rt. Hon. Sir Joseph Banks, *Bart. K. B. P. R. S.* - - - p. 403

XVIII. *Some Observations on the Head of the* Ornithorhynchus paradoxus. *By* Everard Home, *Esq. F. R. S.* - p. 432

PHILOSOPHICAL TRANSACTIONS.

XII. *On double Images caused by atmospherical Refraction.* By William Hyde Wollaston, *M. D. F. R. S.*

Read March 6, 1800.

In some of the last volumes of the Philosophical Transactions, there have been related many instances of strong atmospherical refraction, by which, objects seen near the horizon have appeared inverted, and the horizon itself either elevated or depressed.

Mr. Huddart first took notice of a distinct image, inverted beneath the object itself; and, in the Philosophical Transactions for 1797, has described several such appearances, accompanied with an optical explanation, wherein he shews, that the lowest strata of the air were at the time endued with a weaker refractive power, than others at a small elevation.

In the volume for 1799, Mr. Vince has given an instance (Tab. I. Fig. 1.) where erect, as well as inverted images were visible above, instead of beneath, the objects themselves; and, by tracing the progress of the rays of light, in a manner similar to Mr. Huddart's, concludes that these phænomena arose

from "unusual variations" of increasing density in the lower strata of the atmosphere.

In the volume for 1795, Mr. DALBY mentions having seen "the top of a hill appear detached, for the sky was seen under "it." In this case, as well as in the preceding, it is probable that inversion took place, and that the lower half of the portion detached was an inverted image of the upper, as the sky could not be seen beneath it, but by an inverted course of the rays.

Since the causes of these peculiarities of terrestrial refraction have not received so full an explanation as might be wished, I have endeavoured,

1st. To investigate theoretically the successive variations of increasing or decreasing density to which fluids in general are liable, and the laws of the refractions occasioned by them.

2dly. To illustrate and confirm the truth of this theory, by experiments with fluids of known density.

And lastly, to ascertain, by trial upon the air itself, the causes and extent of those variations of its refractive density, on which the inversions of objects, and other phænomena observed, appear to depend.

The general laws may be comprised in three propositions.

Prop. 1. If the density of any medium varies by parallel indefinitely thin strata, any rays of light moving through it *in the direction of* the strata, will be made to deviate during their passage, and their deviations will be in proportion to the increments of density where they pass.

For each ray will be bent towards the denser strata, by a refracting force proportioned to the difference of the densities above and below the line of its passage; and, as their velocities are the same, and therefore the times of action of the forces

equal, the deviations will be as the refracting forces, *i. e.* as the increments of density.

Prop. II. When two fluids of unequal density are brought into contact, and unite by mutual penetration; if the densities at different heights be expressed by ordinates, the curve which terminates these ordinates will have a point of contrary flexure.

For the straight lines *da*, *rn*, (Plate IX.) Fig. 1. which terminate the ordinates *rx*, *dy*, of uniform density, will be parallel, and, if not united by contrary curvature, some straight line of union, as *ao*, must be supposed. But, from whatever cause the first line *ao* is inferred, by the same cause other intermediate lines *mp*, *tq*, &c. will be produced, and curves *defm*, *mtsr*, will be ultimately formed, having a point of contrary flexure at *m*.

The form of the curve does not appear to admit of accurate investigation, nor is it of importance to the subsequent reasoning, if wholly unknown. We may, however, form some judgment of its nature; for, whether the densities depend on different specific gravities of different fluids, or on unequal temperatures of different portions of the same fluid, the curves will be nearly alike.

In each of these cases, to whatever small distance *pc*, Fig. 2, the mutual attraction is sufficient to occasion intimate union of the fluids, the density *mn* of the mixture will be an arithmetic mean; and, for the same reason, at any intermediate smaller distances, there will be a series of arithmetic means *ef*, *gh*, &c. interposed, and the line *ao*, uniting the ordinates, will be straight.

By progressive effect of this attraction, and successive interpolations, in Fig. 1, curves *defm*, *rstm*, will be formed, of which the straight lines *mp*, *tq*, &c. are tangents.

The attracting distances *np*, *oq*, &c. are subtangents; and, if it be admitted that these are every where equal, the curves so produced are logarithmic, and the increment of the ordinate greatest at *m*, where they meet.

Prop. III. If parallel rays pass through a medium varying according to the preceding proposition, those above the point of contrary flexure will be made to diverge, and those below the same point will converge, after their passage through it.

For, since the deviation of each ray depends on the increment of density where it passes, and since the increment of density is greatest at the point of contrary flexure, any rays, as *ab*, Fig. 3, passing near to that point, will be refracted more towards the denser medium than those at *cd*, which move in a higher stratum, and will diverge from them, but will be refracted towards and meet those at *ef*, which pass nearer to the denser medium, where the increments of density are also less.

Cor. Hence, adjacent portions of the converging rays will form a focus, beyond which they will diverge again; and the varied medium will produce effects similar to those caused by a medium of uniform density* having a surface similar to the curve of densities, since convergence or divergence will be produced, according as the curve of densities is convex or concave; consequently, by tracing backwards, to the extremities of an

* In the varied medium, *bc* and *bm*, Fig. 4, the corresponding increments of the abscissa and ordinate, are to each other as radius to the tangent of the angle *c*. Therefore, the tangent of deviation, which is as the increment of the ordinate, varies as the tangent of the angle *c*.

So also, in the uniform medium, since the sines of refraction and incidence are in a given ratio, their differences will bear a given ratio to either of them; and, when the angles are small, the tangent of deviation will vary as the tangent of incidence, or as the tangent of the angle *c*, which is equal to it.

object, the progress of the visual rays, (or axes of the pencils received by the eye,) it will be manifest that,

Any object seen through the inclined concave part *rm*, Fig. 5. would appear elevated, erect, and somewhat diminished.

An object seen through *md*, where it is convex and inclined, would be elevated; and, if situated beyond the focus of visual rays from the eye, it would appear inverted. The magnitude would depend on the relative distances of the eye and object.

Below the point *d*, where the curve terminates, vision would be direct, so that an object might be situated so as to be seen in all three ways at the same time, direct at O, inverted at I, and erect again at A.

I consider the foregoing propositions as applicable to all cases of varying density, whether occasioned by mutual solution of different fluids, or partial rarefaction of the same fluid; and, by trial of various fluids, however different in density, or even in viscidity, I find that the refractions observe a law agreeable to the theory, as will appear by the following experiments.

Experiment 1. Into a square phial containing a small quantity of clear syrup, I put about an equal quantity of water, in such a way that it floated on the surface of the syrup, without mixing. For a short time, the stratum of union was so thin that nothing could be distinctly seen through it. But soon, by mutual penetration of the water and the syrup, the effects represented at A, Fig. 6, were produced.

Through the syrup, a word written on a card placed behind was seen erect, and in its place; through the adjacent variable medium, an inverted image was visible above the true place; and also, above that, a second image of the same object appeared erect.

When these appearances are first discernible, the variations of density are so great, that the object to be looked at may be in contact with the phial; but, when the variations of density become more gradual, and thereby the focus more distant, any object so near is only elongated, and requires to be removed an inch or two, to be seen inverted.

Exper. 2. Over the surface of the water, in the same phial, I next put about the same measure of rectified spirit of wine.

At the stratum where the water and spirit united, the appearances were the same; but, since the refractive power of spirit exceeds that of water, the true place of the object was seen uppermost; the inverted and erect images are below. Fig. 6, B.

When an oblique line *der* is viewed through any variable medium so made, it appears bent into different forms, according to its situation with respect to focal distance.

If it be at the distance of the principal focus, one point of it is dilated into a vertical line, as *lm*. Fig. 7, A.

If beyond that focus, the portion *lm* is inclined backwards, being an inverted image of *dl*; and *mn* is another image of the same portion seen erect. Fig. 7, B.

On this account, it becomes a convenient object for ascertaining the state of any medium under examination.

In these experiments, the appearances continue many hours, even with spirit of wine; with syrup, two or three days; with acid of vitriol, four or five; with a solution of gum arabic, much longer; but, although their disposition to unite is so different, the appearances produced are nevertheless the same in all.

The refraction is greatest nearly in the plain of original contact of the fluids, diminishing from thence both upwards and downwards. The exact rate of diminution above or below this

point, I had no means of measuring, with the accuracy that would be requisite for determining the nature of the curves of density formed according to the first proposition.

But the truth of the second proposition appeared capable of confirmation by experiment: for the deviation of a ray is there said to depend on the increment of density, and time of the ray's passage, jointly; accordingly, the deviation caused by a given increment should be in proportion to the extent of the medium.

In order to try what effect a greater extent of medium would produce,

Exper. 3. I made a rectangular glass vessel, of which the sides were in the ratio of 10 to 30,6; and, having put into it some clear syrup, with water on its surface, I measured the greatest refractions through it in both directions, and found them in the ratio of 10 to about 29.

In another vessel, of which the sides were as 10 to 40,4, the refractions were, on an average, as 1 to 4.

Being now fully satisfied of the effect of different fluids, I made the following experiment, whereby it appears, that the variations of density occasioned by difference of temperature between adjacent strata of the same fluid, follow a similar law.

Exper. 4. Having put some cold water into a square vessel, I covered its surface with a piece of writing paper perforated with a few small holes, and then filled the vessel cautiously with boiling water. The paper nearly prevented any mixture of the hot and cold water; but, by floating gradually up, left them to communicate their heat by contact alone.

While they were in this state, I examined the appearance of remote objects through the varied medium, and found, that

when my eye was removed four or five feet from the vessel, the effects were the same as in the preceding experiments with different fluids; above any object viewed through the cold water, I could distinguish two images of it, the one inverted, the other erect, as usual; but these appearances did not continue more than five or six minutes.

Having thus established, by experiments sufficiently varied, that the contiguity of two fluids of unequal density is capable of occasioning all the appearances that have been observed, I shall proceed to shew by what means the air may be made to exhibit similar phænomena.

Exper. 5. I heated a common poker red-hot, and held it so as to look along the side of it, at a paper 10 or 12 feet distant. The rarefaction occasioned by it caused a perceptible refraction, to the distance of about $\frac{1}{8}$ of an inch from the side of the poker. A letter seen more distant from it appeared as usual; within that distance there was a faint image of it reversed; and still nearer to the poker was a second image direct, and as distinct as the object itself, but somewhat smaller, as in Fig. 8, in which a section of the atmosphere surrounding the poker is represented. At the bottom and sides it is nearly circular; but upwards the circular form is lost in undulations, occasioned by the rapid ascent of the rarefied air.

The greatest deviation produced in this case measured about $\frac{1}{2}$ a degree.

Exper. 6. By a red-hot bar of iron, 30 inches long, the refractions were much greater, the extreme deviations amounting to full $1\frac{1}{4}$ degree.

The refractions observed in these experiments may, at first view, be thought greater than could be caused by difference of

temperature alone, being in one instance more than double the greatest horizontal refraction of the heavenly bodies; in which case, as the rays enter from a vacuum, the greatest possible effect of the atmosphere might be expected. But it must be remembered, that when a star appears in the horizon, its rays intersect the superior strata of the atmosphere at an inclination of several degrees, and that they pass but once through the variations from rarity to density; but, on the contrary, that in the experiments with red-hot iron, the rays may pass actually in the direction of the strata, and that they are refracted, not only by their entrance from the denser into the rarer medium, but the effect is doubled, since the refraction caused by their emergence is equal to that produced by their incidence.

Although a stratum of air, heated by these means to so great a degree, affords an erect, as well as an inverted, image of objects seen through it, the more moderate warmth communicated to it from bodies heated by the action of the sun upon them, seems insufficient to produce both images; but the inverted image may generally be seen, when the sun shines upon a brick wall, or other darker-coloured surface.

While the eye of the observer is placed nearly in a line with the wall, if another person, at 30 or 40 yards distance, extends any object towards the wall, an image similar to it will appear to come out to meet it.

It would be difficult to ascertain with accuracy the degree of rarefaction capable of shewing this appearance, but it may be of some use to future observers, to mention the different degrees of heat which I observed.

In one instance, a thermometer in contact with the wall, stood at 96°; but, at $\frac{1}{8}$ of an inch distance, 82°.

One morning, when the sun shone bright, I examined the temperatures and refraction produced at the surface of a deal bar painted green, about 8 feet long.

A small thermometer in contact with the bar, rose to 96°; at $\frac{1}{4}$ of an inch distance, it stood at 73°.

The refraction at the same time exceeded 20 minutes.

To explain why red-hot iron occasions two images, while solar heat produces but one, I imagine that the intense heat in the former case rarefies the air for some small distance uniformly, and thereby affords the same series of variations as between other fluids of uniform density; but that, in the latter, the heat is conveyed off as fast as it is generated; so that, as there is no extent of medium uniformly rare, the densities corresponding to the concave portion *rm*, Fig. 5. of the curve before described do not take place, but the phænomena occasioned by the convex part *md* are alone produced.

It must be remarked, that the vertical position of the surface contributes greatly to increase the effect; for, since the heated air rises in the direction of the surface, its ascent has in this case no tendency to blend it with the adjacent denser strata, and hence very different degrees of density take place in the thickness of $\frac{1}{4}$ of an inch; so that, as the increments of density are great, the refractions will be proportionally so; but, where the heated surface is horizontal, the ascent of the rarefied air into the superincumbent denser strata renders the variations far more gradual; consequently, a heated surface of far greater extent must be requisite, to produce equal refraction.

However, over extensive plains, when the sun shines, some degree of inversion is very frequently to be seen; but the inverted images are rarely well defined, unless over a very even

surface. One of the best situations for this purpose, is over a level open road, with a gentle breeze blowing across it. A current of air brings a cool stratum more closely in contact with the heated surface; and, since refraction depends on the increment or difference of density in a given small space, a very moderate breeze will thereby render inversion more perceptible; but a strong wind will reduce the temperature of the surface, and may make the heated stratum too thin for any object to be seen through it from a distance.

In one instance, when I saw a refraction of about 9 minutes, at the distance of $\frac{1}{4}$ of a mile, a thermometer in the sand was 101°; at 4 inches above, 82°; and, at 1 foot above, 76°.

Over water, the evenness of the surface is favourable to the production of such appearances; but, since the action of the sun is weak on a body so transparent, a far greater extent of surface is requisite to produce any perceptible inversion.

Being at Bognor one bright morning, when the sea was calm, I had an opportunity of observing the appearance of Selsea Bill, about 6 miles distant. The whole extent of coast, when viewed with a pocket telescope magnifying about 16 times, appeared inverted from one end to the other; and the lower part of a brick house upon the shore, was seen as distinct as the house itself. I judged the quantity of refraction, in this case, to be about 2 minutes of a degree.

This state of atmosphere appears to be not very uncommon; for, at Shanklin Chine, in the Isle of Wight, a few days preceding, similar appearances were visible in several directions, but I neglected to make any estimate of the quantity of refraction.

In the instance of the inverted vessel seen by Mr. HUDDART,

(Phil. Trans. for 1797, Tab. I. Fig. 3.) at the distance of 8 miles, the refraction seems to have been about 3′.

All the appearances described by him, I am inclined to think, arose from difference of temperature alone. He offers a conjecture, that evaporation might occasion the lower strata of the atmosphere to have a weaker refractive power; but, from the following experiments, it seems to have a contrary effect.

Exper. 7. I took a plate of glass, and, while I looked along the surface, I poured upon it a small quantity of ether.

A line upon the opposite wall, appeared instantaneously elevated many minutes, and at times above $\frac{1}{2}$ a degree.

This fluid being the most volatile, and most soluble in the atmosphere, of any known liquid, produces the greatest effect; since the cold, during evaporation, conspires with the ether dissolved in the air, to increase the refractive power.

Rectified spirit of wine also produces, from the same cause, a very considerable effect.

Exper. 8. By moistening a board, 5 feet in length, with alcohol, and observing the elevation of an object viewed over its surface, I found the refraction to be about 15′.

Exper. 9. I next made a similar experiment with water itself. Of this, the effect was barely visible, when tried in the same way; but, by means of a surface of 10 feet, and by viewing a luminous point at a greater distance, the refraction became evident, and the object elevated above 3 minutes.

In the course of these experiments, I tried whether confining the saturated atmosphere, by boards on each side, would vary the effect, and found the refraction in all cases much lessened; and, when water was used, it became imperceptible; but, as soon as the boards were removed, and a free current allowed to

pass across, the full effect was again produced. The reason of this difference appears to be, that the quicker evaporation increases the degree of cold, and the current brings greater differences of density contiguous.

This state of rapid evaporation will fully account for the phænomenon witnessed by Mr. LATHAM, who has described (in the Phil. Trans. for 1796, p. 357) an extraordinary elevation of the opposite coast of France, so as to be seen from the beach at Hastings, and other parts of Sussex.

There is a fact of the same kind stated by DE LA LANDE, (*Astron.* Tom. II.) who says that the mountains of Corsica (though at the distance of more than 100 miles) are occasionally visible from Genoa.

It is probably owing to the same cause, that other objects have been sometimes seen, at such distances that we should expect them to be intercepted by the curvature of the earth; for it is evident, that whensoever the evaporation over each mile of surface occasions a refraction of about 1 minute, the rays receive a curvature equal to that of the ocean, so that its surface will appear flat, and the spherical form of the earth will not obstruct horizontal vision of objects at any distance.

It still remained to explain the phænomena seen by Mr. VINCE, as I had not hitherto made an atmosphere capable of exhibiting images inverted, as well as elevated, by increased density. For, in the refractions produced in the 7th, 8th, and 9th experiments, by evaporation at an exposed surface, I observed the effect was always greatest in contact with the evaporating surface; any lower point *a*, Fig. 9, appeared brought nearer to a higher point *c*, by the pencil of rays from *a* being more refracted at *b*, than the pencil from *c* was refracted at *d*. Therefore, any rays passing from the eye at *e*, as a point, through

b and *d*, would be made to diverge to *a* and *c*; consequently, visual rays could not, under these circumstances, intersect each other, and no objects could appear inverted.

But, whenever the lowest strata of the air become saturated with moisture, the variations between the saturated stratum and the incumbent atmosphere of the common density, will follow a law similar to what is found at the confines of other fluids of unequal density; hence, inversion will become visible, as there will be a point below which the increment of density will decrease, and where the refractions will consequently be less, although through a denser medium.

Exper. 10. To produce these appearances, I procured a trough of thin deal, 5 feet long, 1 inch wide, with sides 2½ inches high, and closed the extremities of it with glass. A section of it is given in Fig. 10.

When the bottom was wetted with ether, the greatest refraction was, at intervals, more than ¾ of an inch from the bottom of the trough; and, beneath this height, I saw a second image inverted, when my eye was removed to 14 or 15 feet distance, and the object at about 70 feet. The focus seemed at the same time to be about 9 feet distant.

There was not depth enough of uniformly saturated atmosphere for the object itself to be seen through it, but its true place, compared with that of the images, is represented at *a*.

Exper. 11. When I made use of rectified spirit in the same apparatus, I had also sufficient proof that the laws of evaporation would admit of such appearances being produced; for the same object now appeared curved downwards, as in Fig. 11, so that rays nearer to the bottom were manifestly less refracted than such as passed at some distance above. A degree of convergency must therefore have been produced, although the dis-

tance at which the rays would meet, was beyond that of my eye, and circumstances would not admit of my removing beyond 35 feet.

The evaporation of water could not be expected to produce any sensible effect of this kind, in so short a space; but, in a view of some miles extent, there can be no doubt, from the foregoing experiments, that evaporation from the surface of the sea, in such a state of the atmosphere as would allow the lower strata to be saturated, is capable of occasioning all the phænomena which have been described, and probably was the cause of those which Mr. VINCE observed.

Since heat alone tends to depress objects, and evaporation produces apparent elevation, it is probable, that in the instance of refraction related by Mr. DALBY, (Phil. Trans. for 1795, p. 587), the heat of the sun was the principal agent, and that the moisture rather tended to counteract than assist its action.

Simple inversion may generally be seen, when the sun shines upon a *dry* even road of $\frac{1}{3}$ or $\frac{1}{2}$ mile extent; but, when the ground has been wet, I have very rarely seen it, and have even failed of discerning it, when the heat has been sufficient to raise a steam from the ground.

The following experiment shews that it is not to be expected but by very great extent of surface.

Exper. 12. I placed a dark-coloured board in the sunshine, and, having examined the refraction along its surface, I made a wet line along it, with a sponge dipped in boiling water. Notwithstanding this additional heat, the refraction, in the direction of the wet line, was far less than over the rest of the board, although I took care to observe the effect, before the surface could be cooled again by evaporation.

I should therefore expect the depression of the horizon at sea,

where the refraction occasioned by heat must always be counteracted by evaporation, never to exceed a few minutes; and that any one in a situation commanding a view of the sea, by attention to the various degrees of the dip of the horizon under different circumstances, might soon form some estimate of the proper allowance to be made, for brightness of the sun at the time of an astronomical observation, or for difference of temperature between the sea and air.

Having now examined the several peculiarities of refraction which I proposed for consideration, I shall, in few words, recapitulate the purport of the foregoing pages.

According to the theory here given, there appear to be two opposite states of the atmosphere, either of which may occasion objects to be seen doubled or tripled, since both increase and decrease of its density, when partial, produce similar effects.

It has been explained,

1st. Why air heated by the moderate warmth of the sun's rays, occasions objects to appear doubled and inverted.

2dly. Why rarefaction, by a higher degree of heat, gives an additional image, which is not inverted.

3dly. In what state of evaporation the increase of the air's density brings distant objects into view by unusual elevation.

4thly. Under what circumstances evaporation may also produce an inverted image less elevated.

And it is probable, that the same reasoning will afford a ready explanation to other varieties of terrestrial refraction, that may have been, or may hereafter be observed.

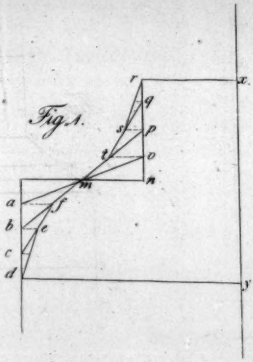


Fig. 1.

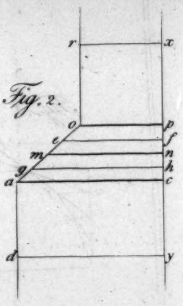
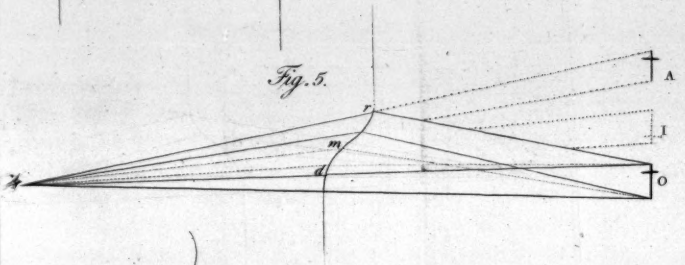


Fig. 2.



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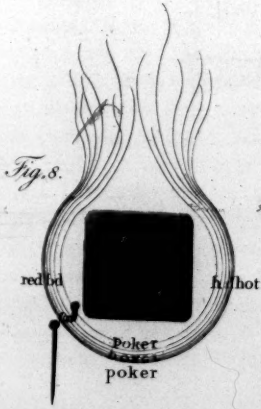


Fig. 8.

Fig. 9.

Fig. 3.

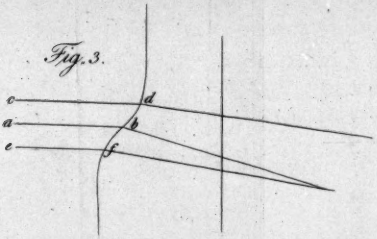


Fig. 4.

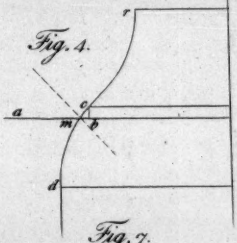


Fig. 6.

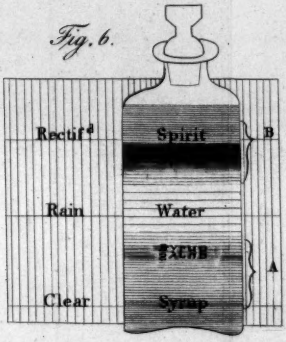


Fig. 7.

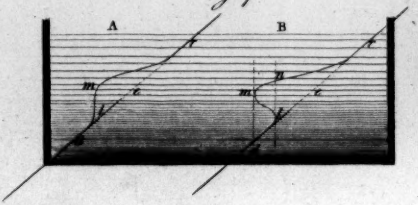


Fig 10.

Fig. 11.

XIII. *Investigation of the Powers of the prismatic Colours to heat and illuminate Objects; with Remarks, that prove the different Refrangibility of radiant Heat. To which is added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers.* By William Herschel, *LL. D. F. R. S.*

Read March 27, 1800.

It is sometimes of great use in natural philosophy, to doubt of things that are commonly taken for granted; especially as the means of resolving any doubt, when once it is entertained, are often within our reach. We may therefore say, that any experiment which leads us to investigate the truth of what was before admitted upon trust, may become of great utility to natural knowledge. Thus, for instance, when we see the effect of the condensation of the sun's rays in the focus of a burning lens, it seems to be natural to suppose, that every one of the united rays contributes its proportional share to the intensity of the heat which is produced; and we should probably think it highly absurd, if it were asserted that many of them had but little concern in the combustion, or vitrification, which follows, when an object is put into that focus. It will therefore not be amiss to mention what gave rise to a surmise, that the power of heating and illuminating objects, might not be equally distributed among the variously coloured rays.

In a variety of experiments I have occasionally made, relating

to the method of viewing the sun, with large telescopes, to the best advantage, I used various combinations of differently-coloured darkening glasses. What appeared remarkable was, that when I used some of them, I felt a sensation of heat, though I had but little light; while others gave me much light, with scarce any sensation of heat. Now, as in these different combinations the sun's image was also differently coloured, it occurred to me, that the prismatic rays might have the power of heating bodies very unequally distributed among them; and, as I judged it right in this respect to entertain a doubt, it appeared equally proper to admit the same with regard to light. If certain colours should be more apt to occasion heat, others might, on the contrary, be more fit for vision, by possessing a superior illuminating power. At all events, it would be proper to recur to experiments for a decision.

Experiments on the heating Power of coloured Rays.

I fixed a piece of pasteboard, AB, (Plate X.) in a frame, mounted upon a stand, CD, and moveable upon two centres. In the pasteboard, I cut an opening, *mn*, a little larger than the ball of a thermometer, and of a sufficient length to let the whole extent of one of the prismatic colours pass through. I then placed three thermometers upon small inclined planes, EF: their balls were blacked with japan ink. That of No. 1 was rather too large for great sensibility. No. 2 and 3 were two excellent thermometers, which my highly esteemed friend Dr. WILSON, late Professor of Astronomy at Glasgow, had lent me for the purpose: their balls being very small, made them of exquisite

sensibility. The scales of all were properly disengaged from the balls.

I now placed the stand, with the framed pasteboard and the thermometers, upon a small plain board, GH; that I might be at liberty to move the whole apparatus together, without deranging the relative situation of the different parts.

This being done, I set a prism, moveable on its axis, into the upper part of an open window, at right angles to the solar ray, and turned it about till its refracted coloured spectrum became stationary, upon a table placed at a proper distance from the window.

The board containing the apparatus was now put on the table, and set in such a manner as to let the rays of one colour pass through the opening in the pasteboard. The moveable frame was then adjusted to be perpendicular to the rays coming from the prism; and the inclined planes carrying the three thermometers, with their balls arranged in a line, were set so near the opening, that any one of them might easily be advanced far enough to receive the irradiation of the colour which passed through the opening, while the rest remained close by, under the shade of the pasteboard.

By repeated trials, I found that Dr. WILSON's No. 2 and mine, always agreed in shewing the temperature of the place where I examined them, when the change was not very sudden; but that mine would require ten minutes to take a change, which the other would shew in five. No. 3 never differed much from No. 2.

1*st Experiment.* Having arranged the three thermometers in the place prepared for the experiment, I waited till they were stationary. Then, advancing No. 1 to the red rays, and leaving

the other two close by, in the shade, I marked down what they shewed, at different times.

No. 1.	No. 2.	No. 3.
$43\frac{1}{2}$	$43\frac{1}{2}$	$43\frac{1}{4}$
48	$43\frac{1}{2}$	$43\frac{1}{2}$
$49\frac{1}{2}$	$43\frac{1}{4}$	$43\frac{1}{4}$
$49\frac{1}{4}$	$43\frac{1}{4}$	$43\frac{1}{4}$
50	$43\frac{1}{4}$	$43\frac{1}{4}$

This, in about 8 or 10 minutes, gave $6\frac{1}{4}$ degrees, for the rising produced in my thermometer, by the red rays, compared to the two standard thermometers.

2d Experiment. As soon as my thermometer was restored to the temperature of the room, which I hastened, by applying it to a large piece of metal that had been kept in the same place, I exposed it again to the red rays, and registered its march, along with No. 2 as a standard, which was as follows.

No. 1.	No. 2.
45	45
48	45
51	45
51	$44\frac{1}{2}$
51	44

Hence, in 10 minutes, the red rays made the thermometer rise 7 degrees.

3d Experiment. Proceeding in the same manner as before, in the green rays I had,

No. 1.		No. 2.
48	- - -	48
45½	- - -	48
46	- - -	48
46	- - -	42¾
46	- - -	42¾

Therefore, in ten minutes, the green rays occasioned a rise of 3¼ degrees.

4th Experiment. I now exposed my thermometer to the violet rays, and compared it with No. 2.

No. 1.		No. 2.
44	- - -	44
44	- - -	44
44¾	- - -	43½
45	- - -	43

Here we have a rising of 2 degrees, in ten minutes, for the violet rays.

5th Experiment. I now exposed Dr. WILSON's thermometer No. 2 to the red rays, and compared its progress with No. 3.

No. 2.		No. 3.
44	- - -	44
46	- - -	44
46½	- - -	43¾
46½	- - -	43¾

Here the thermometer, exposed to red, rose in five minutes 2¼ degrees.

6th Experiment. In red rays again.

No. 2.				No. 3.
44	-	-	-	44
46	-	-	-	44
46½	-	-	-	43½
47	-	-	-	43½
47	-	-	-	43

And here the thermometer, exposed to red, rose in five minutes 4 degrees.

7th Experiment. In green rays.

No. 2.				No. 3.
43½	-	-	-	43½
44½	-	-	-	43½
44½	-	-	-	43

This made the thermometer rise, in the green rays, 1½ degree.

8th Experiment. Again in green rays.

No. 2.				No. 3.
43	-	-	-	43
44½	-	-	-	42¾
44¾	-	-	-	42¾

Here the rising, by the green rays, was 2 degrees.

From these experiments, we are authorised to draw the following results. In the red rays, my thermometer gave 6¾ degrees in the 1st, and 7 degrees in the 2d, for the rising of the quicksilver: a mean of both is 6⅞. In the 3d experiment, we had 3½ degrees, for the rising occasioned by the green rays; from which we obtain the proportion of 55 to 26, for the power of heating in red to that in green. The 4th experiment gave

2 degrees for the violet rays; and therefore we have the rising of the quicksilver in red to that in violet, as 55 to 16.

A sufficient proof of the accuracy of this determination we have, in the result of the four last experiments. The rising for red rays in the 5th, is $2\frac{1}{4}$; and in the 6th, 4 degrees: a mean of both is $3\frac{3}{8}$. In the 7th experiment, we have $1\frac{1}{2}$, and in the 8th, 2 degrees, for the rising in green: a mean of these is $1\frac{3}{4}$. Therefore, we have the proportion of the rising in red to that in green, as 27 to 11, or as 55 to 22,4.

We may take a mean of the result of both thermometers, which will be 55 to 24,2, or more than $2\frac{1}{4}$ to 1, in red to green; and about $3\frac{1}{2}$ to 1, in red to violet.

It appears remarkable, that the most sensible thermometer should give the least alteration, from the exposure to the coloured rays. But since, in these circumstances, there are two causes constantly acting different ways; the one to raise the thermometer, the other to bring it down to the temperature of the room, I suppose, that on account of the smallness of the ball in Dr. WILSON's No. 1, which is but little more than $\frac{1}{8}$ of an inch, the cooling causes must have a stronger effect on the mercury it contains than they can have on mine, the ball of which is half an inch.

More accuracy may hereafter be obtained, by attending to the circumstances of blacking the balls of the thermometers, and their exposure to a more steady and powerful light of the sun, at greater altitudes than it can be had at present; but the experiments which have been related, are quite sufficient for my present purpose; which only goes to prove, that the heating power of the prismatic colours, is very far from being

equally divided, and that the red rays are chiefly eminent in that respect.

Experiments on the illuminating Power of coloured Rays.

In the following examination of the illuminating power of differently-coloured rays, I had two ends in view. The first was, with regard to the illumination itself; and the next, with respect to the aptness of the rays for giving distinct vision; and, though there did not seem to be any particular reason why these two should not go together, I judged it right to attend to both.

The microscope offered itself as the most convenient instrument for this investigation; and I thought it expedient to view only opaque objects, as these would give me an opportunity to use a direct prismatic ray, without running the risk of any bias that might be given to it, in its transmission through the colouring particles of transparent objects.

1*st Experiment.* I placed an object that had very minute parts, under a double microscope; and, having set a prism in the window, so as to make the coloured image of the sun stationary upon the table where the microscope was placed, I caused the differently-coloured rays to fall successively on the object, by advancing the microscope into their light. The magnifying power was 27 times.

In changing the illumination, by admitting a different colour, it always becomes necessary to re-adjust the instrument. It is well known, that the different refrangibility of the rays will

sensibly affect the focal length of object-glasses; but, in compound vision, such as in a microscope, where a very small lens is made to cast a lengthened secondary focus, this difference becomes still more considerable.

By an attentive and repeated inspection, I found that my object was very well seen in red; better in orange, and still better in yellow; full as well in green; but to less advantage in blue; indifferently well in indigo, and with more imperfection in violet.

This trial was made upon one of the microscopic objects which are generally prepared for transparent vision; but, as I used it in the opaque way, I thought that others might be chosen which would answer the purpose better; and, in order to give some variety to my experiments, and to see the effect differently coloured substances might have on the rays of light, I provided the following materials to be viewed. Red paper; green paper; a piece of brass; a nail; a guinea; black paper. Having also found that a higher power might be used, with sufficient convenience for the rays of light to come from the prism to the object, I made the miscroscope magnify 42 times.

The appearance of the nail in the microscope, is so beautiful, that it deserves to be noticed; and the more so, as it is accompanied with circumstances that are very favourable for an investigation, such as that which is under our present consideration. I had chosen it on account of its solidity and blackness, as being most likely to give an impartial result, of the modifications arising from an illumination by differently-coloured rays; but, on viewing it, I was struck with the sight of a bright constellation of thousands of luminous points, scattered over its whole extent, as far as the field of the microscope could take it in.

Their light was that of the illuminating colour, but differed considerably in brightness; some of the points being dim and faint, while others were luminous and brilliant. The brightest of them also, admitted of a little variation in their colour, or rather in the intensity of the same colour; for, in the centre of some of the most brilliant of these lucid appearances, their light had more vivacity, and seemed to deviate from the illuminating tint towards whiteness, while on and near the circumference, it appeared to take a deeper hue.

An object so well divided by nature, into very minute and differently-arranged points, on which the attention might be fixed, in order to ascertain whether they would be equally distinct in all colours, and whether their number would be increased or diminished by different degrees of illumination, was exactly what I wanted; nor could I think it less remarkable, that all the other objects I had fixed upon, besides many more which have been examined, such as copper, tin, silver, *&c.* presented themselves nearly with the same appearance. In the brass, which had been turned in a lathe, the luminous points were arranged in furrows; and in tin they were remarkably beautiful. The result of the examination of my objects was as follows.

2d Experiment. Red paper.

In the red rays, I view a bright point near an accidental black spot in the paper, which serves me as a mark; and I notice the space between the point and the spot: it contains several faint points.

In the orange rays, I see better. The bright point, I now perceive, is double.

In the yellow rays, I see the object still better.

In the green rays, full as well as before.

In the blue rays, very well.

In the indigo rays, not quite so well as in the blue.

In the violet rays, very imperfectly.

3d Experiment. Green paper.

Red. I fix my attention on many faint points, in a space between two bright double points.

Orange. I see those faint points better.

Yellow. Still better.

Green. As well as before. I see remarkably well.

Blue. Less bright, but very distinct.

Indigo. Not well.

Violet. Bad.

4th Experiment. A piece of very clean turned brass.

R. I remark several faint luminous points between two bright ones. The colour of the brass makes the red rays appear like orange.

O. I see better, but the orange colour is likewise different from what it ought to be; however, this is not, at present, the object of my investigation.

Y. I see still better.

G. I see full as well as before.

B. I do not see so well now.

I. I cannot see well.

V. Bad.

5th Experiment. A nail.

R. I remark two bright points, and some faint ones.

O. Brighter than before; and more points visible. Very distinct.

Y. Much brighter than before; and more points and lines visible. Very distinct.

G. Full as bright; and as many points visible. Very distinct.

B. Much less bright. Very distinct.

I. Still less bright. Very distinct.

V. Much less bright again. Very distinct.

6th Experiment. I viewed a guinea, at 9 feet 6 inches from the prism; and adjusted the place of the object in the several rays, by the shadow of the guinea. If this be not done, deceptions will take place.

R. Four remarkable points. Very distinct.

O. Better illuminated. Very distinct.

Y. Still better illuminated. Very distinct. The points all over the field of view are coloured; some green; some red; some yellow; and some white, encircled with black about them.

Between yellow and green is the maximum of illumination. Extremely distinct.

G. As well illuminated as the yellow. Very distinct.

B. Much inferior in illumination. Very distinct.

I. Badly illuminated. Distinct.

V. Very badly illuminated. I can hardly see the object at all.

7th Experiment. The nail again, at 8 feet from the prism.

R. I attended to two bright points, with faint ones between them. Almost all the points in the field of view are red. Very distinct.

O. I see all the points better: they are red, green, yellow, and whitish, with black about them. Very distinct.

Y. I see better. More bright points, and more faint ones: the points are of various colours. Very distinct.

G. I see as well. The points are mostly green, and brightish-green, inclining to white. Very distinct.

B. Much worse illuminated. Very distinct.

I. Badly illuminated. Very distinct.

V. There is hardly any illumination.

8th Experiment. The nail again, at 9 feet 6 inches from the prism, by way of having the rays better separated.

R. Badly illuminated. The bright points are very distinct.

O. Much better illuminated. The bright points very distinct.

Y. Still better illuminated. All points extremely distinct.

G. As well illuminated, and equally distinct.

B. Badly illuminated. The bright points are distinct; but the others are not so.

I. Very badly illuminated. I do not see distinctly; but I believe it to be for want of light.

V. So badly illuminated that I cannot see the object; or at least but barely perceive that it exists.

9th Experiment. Black paper, at 8 feet from the prism.

R. The object is hardly visible. I can only see a few faint points.

O. I see several bright points, and many faint ones.

Y. Numberless bright and small faint points.

Between yellow and green, is the maximum of illumination.

G. The same as the yellow.

B. Very indifferently illuminated; but not so bad as in the red rays.

I. I cannot see the object.

V. Totally invisible.

From these observations, which agree uncommonly well, with respect to the illuminating power assigned to each colour, we may conclude, that the red-making rays are very far from having it in any eminent degree. The orange possess more of it than the red; and the yellow rays illuminate objects still more

perfectly. The maximum of illumination lies in the brightest yellow, or palest green. The green itself is nearly equally bright with the yellow; but, from the full deep green, the illuminating power decreases very sensibly. That of the blue is nearly upon a par with that of the red; the indigo has much less than the blue; and the violet is very deficient.

With regard to the principle of distinctness, there appears to be no deficiency in any one of the colours. In the violet rays, for instance, some of the experiments mention that I saw badly; but this is to be understood only with respect to the number of small objects that could be perceived; for, although I saw fewer of the points, those which remained visible were always as distinct as, in so feeble an illumination, could be expected. It must indeed be evident, that by removing the great obstacle to distinct vision, which is, the different refrangibility of the rays of light, a microscope will be capable of a much higher degree of distinctness than it can be under the usual circumstances. A celebrated optical writer has formerly remarked, that a fly, illuminated by red rays, appeared uncommonly distinct, and that all its minute parts might be seen in great perfection; and, from the experiments which have been related, it appears that every other colour is possessed of the same advantage.

I am well aware that the results I have drawn from the foregoing experiments, both with regard to the heating and illuminating powers of differently-coloured rays, must be affected by some little inaccuracies. The prism, under the circumstances in which I have used it, could not effect a complete separation of the colours, on account of the apparent diameter of the sun, and the considerable breadth of the prism itself, through which the rays were transmitted.

Perhaps an arrangement like that in Fig. 16, of the NEWTONIAN experiments, might be employed; if instruments of sufficient sensibility, such as air thermometers, can be procured, that may be affected by the enfeebled illumination of rays that have undergone four transmissions, and eight refractions; and especially when their incipient quantity has been so greatly reduced, in their limited passage through a small hole at the first incidence.

But it appeared most expedient for me, at present, to neglect all further refinements, which may be attempted hereafter at leisure. It may even be presumed that, had there not been some small admixture of the red rays in the other colours, the result would have been still more decisive, with regard to the power of heating vested in the red rays. And it is likewise evident, that at least the red light of the prismatic spectrum, was much less adulterated than any of the other colours; their refractions tending all to throw them from the red. That the same rays which occasion the greatest heat, have not the power of illumination in any strong degree, stands on as good a foundation. For, since here also they have undergone the fairest trial, as being most free from other colours, it is equally proved that they illuminate objects but imperfectly. There is some probability that a ray, purified in the NEWTONIAN manner above quoted, especially in a well darkened room, may remain bright enough to serve the purpose of microscopic illumination, in which case, more precision can easily be obtained.

The greatest cause for a mixture of colours, however, which is, the breadth of the prism, I saw might easily be removed; therefore, on account of the coloured points, which have been mentioned in the 6th and 7th experiments, I was willing to try

whether they proceeded from this mixture; and therefore covered the prism in front with a piece of pasteboard, having a slit in it of about $\frac{1}{10}$ of an inch broad.

10*th Experiment.* The nail, at 9 feet 2 inches from the prism.

R. I fix my attention on two shining, red points; they are pretty bright.

O. I see many more points. The object is better illuminated than in the red. The points are surrounded by black; but are orange-coloured.

Y. The points now are yellow, and white surrounded by black. The object is better illuminated than in orange.

The maximum of illumination is in the brightest yellow, or palest green.

G. The points are green and white, as before surrounded by black. Better illuminated than in orange.

B. The illumination is nearly equal to red.

I. Very indifferently illuminated.

V. Very badly illuminated.

The phænomena of the differently-coloured points being now completely resolved, since they were plainly owing to the former admixture of colours, and the illuminating power remaining ascertained as before, I attempted also to repeat the experiments upon the thermometer, with the prism covered in the same manner; but I found the effect of the coloured rays too much enfeebled to give a decisive result.

I might now proceed to my next subject; but it may be pardonable if I digress for a moment, and remark, that the foregoing researches ought to lead us on to others. May not the chemical properties of the prismatic colours be as different as those which relate to light and heat? Adequate methods for an investigation

of them may easily be found; and we cannot too minutely enter into an analysis of light, which is the most subtle of all the active principles that are concerned in the mechanism of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our daily observation, but which have hitherto remained unexplained. If the power of heating, as we now see, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the state of a red glow; and for the heat which is given by charcoal, coke, and balls of small-coal mixed up with clay, used in hot-houses; all which, it is well known, throw out red light. It also explains the reason why the yellow, green, blue, and purple flames of burning spirits mixed with salt, occasion so little heat that a hand is not materially injured, when passed through their coruscations. If the chemical properties of colours also, when ascertained, should be such that an acid principle, for instance, which has been ascribed to light in general, on account of its changing the complexion of various substances exposed to it, may reside only in one of the colours, while others may prove to be differently invested, it will follow, that bodies may be variously affected by light, according as they imbibe and retain, or transmit and reflect, the different colours of which it is composed.

Radiant Heat is of different Refrangibility.

I must now remark, that my foregoing experiments ascertain beyond a doubt, that radiant heat, as well as light, whether they be the same or different agents, is not only refrangible,

but is also subject to the laws of the dispersion arising from its different refrangibility; and, as this subject is new, I may be permitted to dwell a few moments upon it. The prism refracts radiant heat, so as to separate that which is less efficacious, from that which is more so. The whole quantity of radiant heat contained in a sun-beam, if this different refrangibility did not exist, must inevitably fall uniformly on a space equal to the area of the prism; and, if radiant heat were not refrangible at all, it would fall upon an equal space, in the place where the shadow of the prism, when covered, may be seen. But, neither of these events taking place, it is evident that radiant heat is subject to the laws of refraction, and also to those of the different refrangibility of light. May not this lead us to surmise, that radiant heat consists of particles of light of a certain range of momenta, and which range may extend a little farther, on each side of refrangibility, than that of light? We have shewn, that in a gradual exposure of the thermometer to the rays of the prismatic spectrum, beginning from the violet, we come to the maximum of light, long before we come to that of heat, which lies at the other extreme. By several experiments, which time will not allow me now to report, it appears that the maximum of illumination has little more than half the heat of the full red rays; and, from other experiments, I likewise conclude, that the full red falls still short of the maximum of heat; which perhaps lies even a little beyond visible refraction. In this case, radiant heat will at least partly, if not chiefly, consist, if I may be permitted the expression, of invisible light; that is to say, of rays coming from the sun, that have such a momentum as to be unfit for vision. And, admitting, as is highly probable, that the organs of sight are only adapted to receive impressions from

particles of a certain momentum, it explains why the maximum of illumination should be in the middle of the refrangible rays; as those which have greater or less momenta, are likely to become equally unfit for impressions of sight. Whereas, in radiant heat, there may be no such limitation to the momentum of its particles. From the powerful effects of a burning lens, however, we gather the information, that the momentum of terrestrial radiant heat is not likely to exceed that of the sun; and that, consequently, the refrangibility of *calorific* rays cannot extend much beyond that of *colourific* light. Hence we may also infer, that the invisible heat of red-hot iron, gradually cooled till it ceases to shine, has the momentum of the invisible rays which, in the solar spectrum viewed by day-light, go to the confines of red; and this will afford an easy solution of the reflection of invisible heat by concave mirrors.

Application of the Result of the foregoing Observations, to the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers.

Some time before the late transit of Mercury over the disk of the sun, I prepared my 7-feet telescope, in order to see it to the best advantage. As I wished to keep the whole aperture of the mirror open, I soon cracked every one of the darkening slips of wedged glasses, which are generally used with achromatic telescopes: none of them could withstand the accumulated heat in the focus of pencils, where these glasses are generally placed. Being thus left without resource, I made use of red glasses; but was by no means satisfied with their performance.

My not being better prepared, as it happened, was of no consequence; the weather proving totally unfavourable for viewing the sun at the time of the transit. However, as I was fully aware of the necessity of providing an apparatus for this purpose, since no method that was in use could be applied to my telescopes, I took the first opportunity of beginning my trials.

The instrument I wished to adapt for solar inspection, was a NEWTONIAN reflector, with 9 inches aperture; and my aim was, to use the whole of it open.

I began with a red glass; and, not finding it to stop light enough, took two of them together. These intercepted full as much light as was necessary; but I soon found that the eye could not bear the irritation, from a sensation of heat, which it appeared these glasses did not stop.

I now took two green glasses; but found that they did not intercept light enough. I therefore smoked one of them; and it appeared that, notwithstanding they now still transmitted considerably more light than the red glasses, they remedied the former inconvenience of an irritation arising from heat. Repeating these trials several times, I constantly found the same result; and, the sun in the first case being of a deep red colour, I surmised that the red-making rays, transmitted through red glasses, were more efficacious in raising a sensation of heat, than those which passed through green, and which caused the sun to look greenish. In consequence of this surmise, I undertook the investigations which have been delivered under the two first heads.

As soon as I was convinced that the red light of the sun ought to be intercepted, on account of the heat it occasions, and that it might also be safely set aside, since it was now proved

that pale green light excels in illumination, the method which ought to be pursued in the construction of a darkening apparatus was sufficiently pointed out; and nothing remained but to find such materials as would give us the colour of the sun, viewed in a telescope, of a pale green light, sufficiently tempered for the eye to bear its lustre.

To determine what glasses would most effectually stop the red rays, I procured some of all colours, and tried them in the following manner.

I placed a prism in the upper part of a window, and received its coloured spectrum upon a sheet of white paper. Then I intercepted the colours, just before they came to the paper, successively, by the glasses, and found the result as follows.

A deep red glass intercepted all the rays.

A paler red did the same.

From this, we ought not to conclude that red glasses will stop the red rays; but rather, that none of the sun's light, after its dispersion by the prism, remains intense enough to pass through red glasses, in sufficient quantity to be perceptible, when it comes to the paper. By looking through them directly at the sun, or even at day objects, it is sufficiently evident that they transmit chiefly red rays.

An orange glass transmitted nearly all the red, the orange, and the yellow. It intercepted some of the green; much of the blue; and very little of the indigo and violet.

A yellow glass intercepted hardly any light, of any one of the colours.

A dark green glass intercepted nearly all the red, and partly also the orange and yellow. It transmitted the green; intercepted much of the blue; but none of the indigo and violet.

A darker green glass intercepted nearly all the red; much of the orange; and a little of the yellow. It transmitted the green; stopped some of the blue; but transmitted the indigo and violet.

A blue glass intercepted much of the red and orange; some of the yellow; hardly any of the green; none of the blue, indigo, or violet.

A purple glass transmitted some of the red; a very little of the orange and yellow: it also transmitted a little of the green and blue; but more of the indigo and violet.

From these experiments we see, that dark green glasses are most efficacious for intercepting red light, and will therefore answer one of the intended purposes; but since, in viewing the sun, we have also its splendour to contend with, I proceeded to the following additional trials.

White glass, lightly smoked, apparently intercepted an equal share of all the colours; and, when the smoke was laid on thicker, it permitted none of them to pass.

Hard pitch, melted between two white glasses, intercepted much light; and, when put on sufficiently thick, transmitted none.

Many differently-coloured fluids, that were also tried, I found were not sufficiently pure to be used, when dense enough to stop light.

Now, red glasses, and the two last-mentioned resources of smoke, and pitch, any one of which, it has been seen, will stop as much light as may be required, had still a remaining trial to undergo, relating to distinctness; but this I was convinced could only be decided by actual observations of the sun.

As an easy way of smoking glasses uniformly is of some

consequence to distinct vision, it may be of service here to give the proper directions, how to proceed in the operation.

With a pair of warm pliers, take hold of the glass, and place it over a candle, at a sufficient distance not to contract smoke. When it is heated, but no more than still to permit a finger to touch the edges of it, bring down the glass, at the side of the flame, as low as the wick will permit, which must not be touched. Then, with a quick vibratory motion, agitate it in the flame from side to side; at the same time advancing and retiring it gently all the while. By this method, you may proceed to lay on smoke to any required darkness. It ought to be viewed from time to time, not only to see whether it be sufficiently dark, but whether any inequality may be perceived; for, if that should happen, it will not be proper to go on.

The smoke of sealing-wax is bad: that of pitch is worse. A wax candle gives a good smoke; but that of a tallow candle is better. As good as any I have hitherto met with, is the smoke of spermaceti oil. In using a lamp, you may also have the advantage of an even flame extended to any length.

Telescopic Experiments.

No. 1. By way of putting my theory to the trial, I used two red glasses, and found that the heat which passed through them could not be suffered a moment; but I was now also convinced that distinctness of vision is capitally injured, by the colouring matter of these glasses.

No. 2. I smoked a white glass, till it stopped light enough to

permit the eye to bear the sun. This destroyed all distinctness; and also permitted some heat to come to the eye, by transmitting chiefly red rays.

No. 3. I applied two white glasses, with pitch between them, to the telescope; and found that it made the sun appear of a scarlet colour. They transmitted some heat; and distinctness was greatly injured.

No 4. I used a very dark green glass, to stop heat; and behind it, or towards the eye, I placed a red glass, to stop light. The first glimpse I had of the sun, was accompanied with a sensation of heat; distinctness also was materially injured.

No. 5. I used a dark green and a pale red; but, the sun not being sufficiently darkened, I smoked the red glass, and, putting a small partition between the two, placed the smoke towards the green glass. This took off the exuberance of light; but did not remedy the inconvenience arising from heat.

No. 6. I used two pale green glasses; smoking that next to the eye, and placing it as in No. 5, so that the smoke might be inclosed between the two. This acted incomparably well; but, in a very short time, the heat which passed the first glass, (though not the second, for I felt no sensation of it in the eye,) disordered the smoke, by drawing it up into little blisters or stars, which let through light; and this composition, therefore, soon became useless.

No. 7. I used two dark green glasses, one of them smoked, as in No. 5. These also acted well; but became useless, for the reason assigned in No. 6, though somewhat less smoke had been required than in the former composition. I felt no heat.

No. 8. I used one pale green, with a dark green smoked glass upon it, as in No. 5. It bore an aperture of 4 inches very

well, and the smoke was not disordered; but, when all the tube was open, the pale green glass cracked in a few minutes.

No. 9. Placing now a dark green before a smoked green, I saw the sun remarkably well. In this experiment, I had made a difference in the arrangement of the apparatus. The cracking of the glasses, I supposed, might be owing to their receiving heat in the middle, while the outside remained cold; which would occasion a partial dilatation. I therefore cut them into pieces about a quarter of an inch square, and set three of them in a slider, so that I could move them behind the smoked glass, without disturbing it. After looking about three or four minutes through one of them, I moved the slider to the second, and then to the third. This kept the glasses sufficiently cool; but the disturbance of the alterations proved hurtful to vision, which requires repose; and, if perchance I stopped a little longer than the proper time, the glass cracked, with a very disagreeable explosion, that endangered the eye.

No. 10. Two dark green glasses, both smoked, that a thinner coat might be on each; but the smoke still contracted blisters, though less dense than before.

No. 11. To get rid of smoke intirely, I used two dark green glasses, two very dark green, two pale blue, and one pale green glass, together. Distinctness was wanting; nor was light sufficiently intercepted.

No. 12. A dark green and a pale blue glass, smoked. The green glass cracked.

No. 13. A pale blue and a dark green glass, smoked. The blue glass cracked. The eye felt no sensation of heat.

No. 14. Two pale blue glasses, one smoked. The first glass cracked.

It was now sufficiently evident, that no glass which stops heat, and therefore receives it, could be preserved from cracking, when exposed to the focus of pencils. This induced me to try an application of the darkening apparatus to another part of the telescope.

The place where the rays are least condensed, without interfering with the reflections of the mirrors, is immediately close to the small one. I therefore screwed an apparatus to the speculum arm, into which any glass might be placed.

No. 15. A dark green glass close to the small speculum, and smoked pale green in the focus of pencils, as before. I saw remarkably well.

No. 16. The dark green as before; but, that more light might be admitted, a white smoked glass near the eye. Better than No. 15; but the green glass cracked.

No. 17. A very dark green and white smoked glass, as before. Very distinct, but the green glass cracked in about six or seven minutes.

No. 18. A dark blue glass, as in No. 15, and white smoked. This was distinct; and no heat came to the eye. The sun appeared ruddy.

No. 19. A dark blue and a yellow glass, close together, as in No. 15, and a white smoked one, as before. This was not distinct.

No. 20. A purple glass, as in No. 15, with a white smoked one. This gave the sun of a deep orange colour, approaching to scarlet. It was not distinct.

No. 21. An orange glass, as in No. 15, with a white smoked one. The colour of the sun was too red.

No. 22. A white smoked glass, as in No. 15, without any

other at the eye. This gave the sun of a beautiful orange colour; but distinctness was totally destroyed.

No. 23. The heat near the small speculum being still too powerful for the glasses, I had a bluish dark green glass made of a proper diameter to be inclosed between the two eye-glasses of a double eye-piece. All glass I knew would stop some heat; and was therefore in hopes that the interposition of this eye-glass would temper the rays, so as in some measure to protect the coloured glass. In the usual place near the eye, I put two white glasses, with a thin coat of pitch between them. These glasses, when looked through by the natural eye, give the sun of a red colour; I therefore entertained no great hopes of their application to the telescope. They darkened the sun not sufficiently; and, when the pitch was thickened, distinctness was wanting.

No. 24. The same glass between the eye-glasses, and a dark green smoked glass at the eye. Very distinct. This arrangement is preferable to that of No. 15; after some considerable time, however, this glass also cracked.

No. 25. I placed a very dark green glass behind the second eye-glass, that it might be sheltered by both glasses, which in my double eye-piece are close together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glass receives them on a large surface, and stops light and heat, in the proportion of the squares of its diameter now used, to that on which the rays would have fallen, had it been placed in the focus of pencils. And, for the same reason, I now also placed a dark green smoked glass close upon the former, with the smoked side towards the eye, that the smoke might

likewise be protected against heat, by a passage of the rays through two surfaces of coloured glass.

This position had moreover the advantage of leaving the telescope, with its mirrors and glasses, completely to perform its operation, before the application of the darkening apparatus; and thus to prevent the injury which must be occasioned, by the interposition of the heterogeneous colouring matter of the glasses and of the smoke.

No. 26. I placed a deep blue glass with a bluish green smoked one upon it, as in No. 25, and found the sun of a whiter colour than with the former composition. There was no disagreeable sensation of heat; though a little warmth might be felt.

No. 27. I used two black glasses, placed as in No. 25. Here there was no occasion for smoke; but the sun appeared of a bright scarlet colour, and an intolerable sensation of heat took place immediately. I rather suspect that these are very deep red glasses, though their outward appearance is black.

In order to have a more sure criterion of heat, I applied Dr. WILSON'S thermometer, No. 2, to the end of the eye-piece, where the eye is generally placed. With No. 25, it rose from 34 to 37 degrees. With No. 26, it rose from 35 to 46; and, with No. 27, it rose, very quickly, from 96 to 95 degrees. I am pretty sure it would have mounted up still higher; but, the scale extending only to 100, I was not willing to run the risk of breaking the thermometer by a longer exposure.

It remains now only to be added, that with No. 25 and 26 I have seen uncommonly well; and that, in a long series of very interesting observations upon the sun, which will soon be

communicated, the glasses have met with no accident. However, when the sun is at a considerable altitude, it will be advisable to lessen the aperture a little, in telescopes that have so much light as my 10-feet reflector; or, which will give us more distinctness, to view the sun earlier in the morning, and later in the afternoon; for, the light intercepted by the atmosphere in lower altitudes, will reduce its brilliancy much more uniformly than we can soften it, by laying on more smoke upon our darkening glasses. Now, as few instruments in common use are so large as that to which this method of darkening has been adapted, we may hope that it will be of general utility in solar observations.

Slough, near Windsor,
March 8, 1800.

XIV. *Experiments on the Refrangibility of the invisible Rays of the Sun.* *By* William Herschel, *LL. D. F. R. S.*

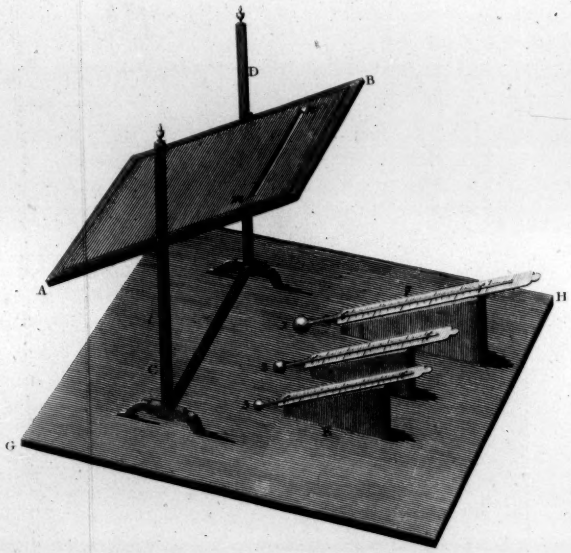
Read April 24, 1800.

In that section of my former paper which treats of radiant heat, it was hinted, though from imperfect experiments, that the range of its refrangibility is probably more extensive than that of the prismatic colours; but, having lately had some favourable sunshine, and obtained a sufficient confirmation of the same, it will be proper to add the following experiments to those which have been given.

I provided a small stand, with four short legs, and covered it with white paper.* On this I drew five lines, parallel to one end of the stand, at half an inch distance from each other, but so that the first of the lines might only be $\frac{1}{4}$ of an inch from the edge. These lines I intersected at right angles with three others; the 2d and 3d whereof were, respectively, at $2\frac{1}{2}$ and at 4 inches from the first.

The same thermometers that have before been marked No. 1, 2, and 3, mounted upon their small inclined planes, were then placed so as to have the centres of the shadow of their balls thrown on the intersection of these lines. Now, setting my little stand upon a table, I caused the prismatic spectrum to fall with its extreme colour upon the edge of the paper, so that none might advance beyond the first line. In this arrangement,

* See Plate XI.



Basire sc.

all the spectrum, except the vanishing last quarter of an inch, which served as a direction, passed down by the edge of the stand, and could not interfere with the experiments. I had also now used the precaution of darkening the window in which the prism was placed, by fixing up a thick dark green curtain, to keep out as much light as convenient.

The thermometers being perfectly settled at the temperature of the room, I placed the stand so that part of the red colour, refracted by the prism, fell on the edge of the paper, before the thermometer No. 1, and about half way, or $1\frac{1}{4}$ inch, towards the second: it consequently did not come before that, or the 3d thermometer, both which were to be my standards. During the experiment, I kept the last termination of visible red carefully to the first line, as a limit assigned to it, by gently moving the stand when required; and found the thermometers, which were all placed on the second line, affected as follows.

No. 1.	No. 2.	No. 3.
45	45	44
49	45	44
51	$44\frac{3}{4}$	44
$50\frac{1}{4}$	$43\frac{3}{4}$	$43\frac{1}{2}$

Here the thermometer No. 1 rose $6\frac{1}{2}$ degrees, in 10 minutes, when its centre was placed $\frac{1}{2}$ inch beyond visible light.

In order to have a confirmation of this fact, I cooled the thermometer No. 1, and placed No. 2 in the room of it: I also put No. 3 in the place of No. 2, and No. 1 in that of No. 3; and, having exposed them as before, arranged on the second line, I had the following result.

No. 2.	No. 3.	No. 1.
44	44	45
47	44	45
$46\frac{3}{4}$	44	45
$46\frac{3}{4}$	44	45

Here the thermometer No. 2 rose $2\frac{3}{4}$ degrees, in 12 minutes; and being, as has been noticed before, much more sensible than No. 1, it came to the temperature of its situation in a short time; but I left it exposed longer, on purpose to be perfectly assured of the result. Its shewing but $2\frac{3}{4}$ degrees advance, when No. 1 shewed $6\frac{1}{2}$, has also been accounted for before.

It being now evident that there was a refraction of rays coming from the sun, which, though not fit for vision, were yet highly invested with a power of occasioning heat, I proceeded to examine its extent as follows.

The thermometers were arranged on the third line, instead of the second; and the stand was, as before, immersed up to the first, in the coloured margin of the vanishing red rays. The result was thus.

No. 1.	No. 2.	No. 3.
46	46	$45\frac{3}{4}$
50′	$46\frac{1}{2}$	46
$51\frac{1}{4}$	$46\frac{3}{4}$	$46\frac{1}{4}$
$52\frac{1}{4}$	47	$46\frac{3}{4}$

Here the thermometer No. 1 rose $5\frac{1}{4}$ degrees, in 13 minutes, at 1 inch behind the visible light of the red rays.

I placed now the thermometers on the 4th line, instead of the 3d; and, proceeding as before, I had the following result.

No. 1.			No. 2.			No. 3.
$48\frac{1}{4}$	-	-	$48\frac{1}{4}$	-	-	$47\frac{1}{4}$
$51\frac{1}{2}$	-	-	$48\frac{3}{8}$	-	-	$47\frac{7}{8}$

Therefore, the thermometer No. 1 rose $3\frac{1}{8}$ degree, in 10 minutes, at $1\frac{1}{2}$ inch beyond the visible light of the red rays.

I might now have gone on to the 5th line; but, so fine a day, with regard to clearness of sky and perfect calmness, was not to be expected often, at this time of the year; I therefore hastened to make a trial of the other extreme of the prismatic spectrum. This was attended with some difficulty, as the illumination of the violet rays is so feeble, that a precise termination of it cannot be perceived. However, as well as could be judged, I placed the thermometers one inch beyond the reach of the violet rays, and found the result as follows.

No. 1.			No. 2.			No. 3.
48	-	-	48	-	-	$47\frac{3}{4}$
48	-	-	48	-	-	$47\frac{3}{4}$
48	-	-	$47\frac{1}{2}$	-	-	47
$48\frac{1}{2}$	-	-	$47\frac{1}{2}$	-	-	47
48	-	-	48	-	-	$47\frac{3}{4}$

Here the several indications of the thermometers, two of which, No. 1 and 2, were used as variable, while the 3d was kept as the standard, were read off during a time that lasted 12 minutes; but they afford, as may be seen by inspection, no ground for ascribing any of their small changes to other causes than the accidental disturbance which will arise from the motion of the air, in a room where some employment is carried on.

I exposed the thermometer now to the line of the very first perceptible violet light; but so that No. 1 and 2 might again

be in the illumination, while No. 3 remained a standard. The result proved as follows.

No. 1.	No. 2.	No. 3.
48	48	$47\frac{3}{4}$
$48\frac{1}{2}$	48	$47\frac{3}{4}$
$48\frac{3}{4}$	$48\frac{1}{2}$	$47\frac{3}{4}$
49	$48\frac{1}{2}$	$47\frac{3}{4}$

Here the thermometer No. 1 rose 1 degree, in 15 minutes; and No. 2 rose $\frac{1}{2}$ degree, in the same time.

From these last experiments, I was now sufficiently persuaded, that no rays which might fall beyond the violet, could have any perceptible power, either of illuminating or of heating; and that both these powers continued together throughout the prismatic spectrum, and ended where the faintest violet vanishes.

A very material point remained still to be determined, which was, the situation of the maximum of the heating power.

As I knew already that it did not lie on the violet side of the red, I began at the full red colour, and exposed my thermometers, arranged on a line, so as to have the ball of No. 1 in the midst of its rays, while the other two remained at the side, unaffected by them.

No. 1.	No. 2.	No. 3.
$48\frac{1}{2}$	$48\frac{1}{2}$	48
$55\frac{1}{2}$	$48\frac{1}{2}$	48
$55\frac{1}{2}$	$48\frac{1}{2}$	48

Here the thermometer No. 1 rose 7 degrees, in 10 minutes, by an exposure to the full red coloured rays.

I drew back the stand, till the centre of the ball of No. 1 was

just at the vanishing of the red colour, so that half its ball was within, and half without, the visible rays of the sun.

No. 1.	No. 2.	No. 3.
$48\frac{1}{2}$	$48\frac{1}{2}$	48
$55\frac{1}{2}$	$48\frac{1}{2}$	48
57	49	$48\frac{1}{2}$

Here the thermometer No. 1 rose 8 degrees, in 10 minutes.

By way of not losing time, in order to connect these last observations the better together, I did not bring back the thermometer No. 1 to the temperature of the room, being already well acquainted with its rate of shewing, compared to that of No. 2, but went on to the next experiment, by withdrawing the stand, till the whole ball of No. 1 was completely out of the sun's visible rays, yet so as to bring the termination of the line of the red colour as near the outside of the ball as could be, without touching it.

No. 1.	No. 2.	No. 3.
57	49	$48\frac{1}{2}$
$58\frac{1}{2}$	$49\frac{3}{4}$	49
59	$50\frac{1}{4}$	$49\frac{3}{4}$
59	50	$49\frac{1}{2}$

Here the thermometer No. 1 rose, in 10 minutes, another degree higher than in its former situation it could be brought up to; and was now 9 degrees above the standard. The ball of this thermometer, as has been noticed, is exactly half an inch in diameter; and its centre therefore was $\frac{1}{4}$ inch beyond the visible illumination, to which no part of it was exposed.

It would not have been proper to compare these last observations with those taken at an earlier period this morning, in

order to obtain a true maximum, as the sun was now more powerful than it had been at that time; for which reason, I caused the line of termination of visible light, now to fall again just $\frac{1}{2}$ inch from the centre of the ball; and had the following result.

No. 1.	No. 2.	No. 3.
$50\frac{1}{2}$	$50\frac{1}{2}$	50
$57\frac{3}{4}$	50	$49\frac{1}{2}$
$58\frac{1}{2}$	50	$49\frac{1}{2}$
$58\frac{3}{4}$	50	$49\frac{1}{2}$

And here the thermometer No. 1 rose, in 16 minutes, $8\frac{3}{4}$ degrees, when its centre was $\frac{1}{2}$ inch out of the visible rays of the sun. Now, as before we had a rising of 9 degrees, and here $8\frac{3}{4}$, the difference is almost too trifling to suppose, that this latter situation of the thermometer was much beyond the maximum of the heating power; while, at the same time, the experiment sufficiently indicates, that the place inquired after need not be looked for at a greater distance.

It will now be easy to draw the result of these observations into a very narrow compass.

The first four experiments prove, that there are rays coming from the sun, which are less refrangible than any of those that affect the sight. They are invested with a high power of heating bodies, but with none of illuminating objects; and this explains the reason why they have hitherto escaped unnoticed.

My present intention is, not to assign the angle of the least refrangibility belonging to these rays, for which purpose more accurate, repeated, and extended experiments are required. But, at the distance of 52 inches from the prism, there was still a considerable heating power exerted by our invisible rays, one

inch and a half beyond the red ones, measured upon their projection on a horizontal plane. I have no doubt but that their efficacy may be traced still somewhat farther.

The 5th and 6th experiments shew, that the power of heating is extended to the utmost limits of the visible violet rays, but not beyond them; and that it is gradually impaired, as the rays grow more refrangible.

The four last experiments prove, that the maximum of the heating power is vested among the invisible rays; and is probably not less than half an inch beyond the last visible ones, when projected in the manner before mentioned. The same experiments also shew, that the sun's invisible rays, in their less refrangible state, and considerably beyond the maximum, still exert a heating power fully equal to that of red-coloured light; and that, consequently, if we may infer the quantity of the efficient from the effect produced, the invisible rays of the sun probably far exceed the visible ones in number.

To conclude, if we call *light*, those rays which illuminate objects, and *radiant heat*, those which heat bodies, it may be inquired, whether light be essentially different from radiant heat? In answer to which I would suggest, that we are not allowed, by the rules of philosophizing, to admit two different causes to explain certain effects, if they may be accounted for by one. A beam of radiant heat, emanating from the sun, consists of rays that are differently refrangible. The range of their extent, when dispersed by a prism, begins at violet-coloured light, where they are most refracted, and have the least efficacy. We have traced these calorific rays throughout the whole extent of the prismatic spectrum; and found their power increasing, while their refrangibility was lessened, as far

as to the confines of red-coloured light. But their diminishing refrangibility, and increasing power, did not stop here; for we have pursued them a considerable way beyond the *prismatic spectrum*, into an invisible state, still exerting their increasing energy, with a decrease of refrangibility up to the maximum of their power; and have also traced them to that state where, though still less refracted, their energy, on account, we may suppose, of their now failing density, decreased pretty fast; after which, the invisible *thermometrical spectrum*, if I may so call it, soon vanished.

If this be a true account of solar heat, for the support of which I appeal to my experiments, it remains only for us to admit, that such of the rays of the sun as have the refrangibility of those which are contained in the prismatic spectrum, by the construction of the organs of sight, are admitted, under the appearance of light and colours; and that the rest, being stopped in the coats and humours of the eye, act upon them, as they are known to do upon all the other parts of our body, by occasioning a sensation of heat.

Slough, near Windsor,
March 17, 1800.

EXPLANATION OF PLATE XI.

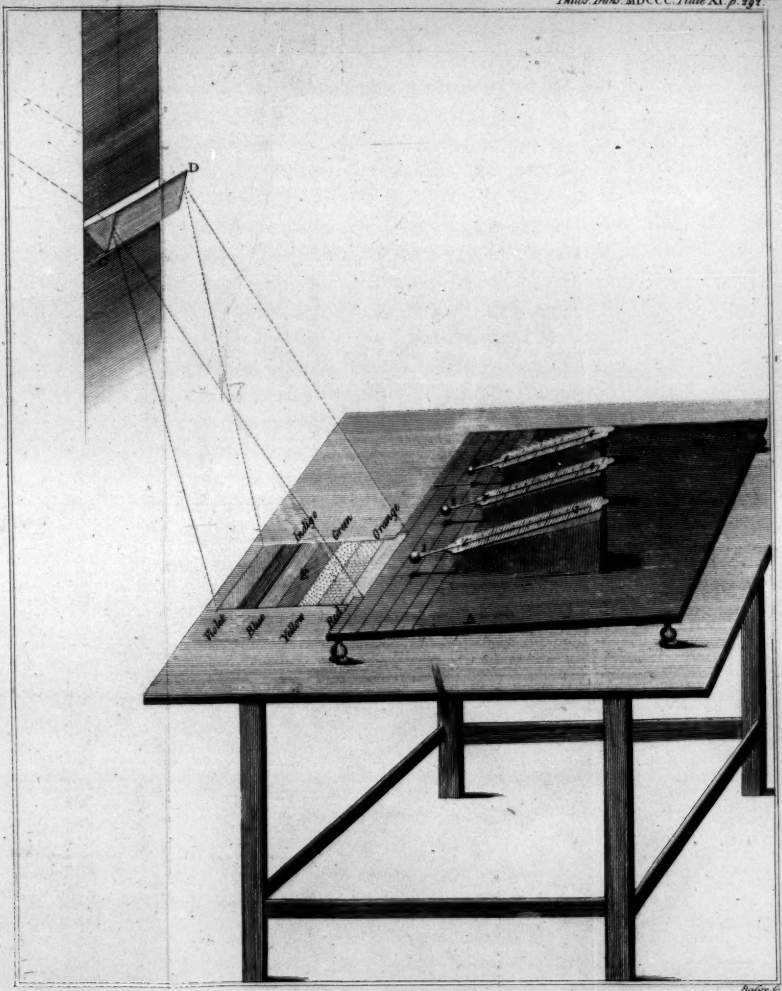
IN WHICH IS GIVEN A VIEW OF THE APPARATUS.

A B. The small stand.

1, 2, 3. The thermometers upon it.

C D. The prism at the window.

E. The spectrum thrown upon the table, so as to bring the last quarter of an inch of the red colour upon the stand.



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XV. *Experiments on the solar, and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different.* By William Herschel, *LL. D. F. R. S.*

Part I.

Read May 15, 1800.

The word heat, in its most common acceptation, denotes a certain sensation, which is well known to every person. The cause of this sensation, to avoid ambiguity, ought to have been distinguished by a name different from that which is used to point out its effect. Various authors indeed, who have treated on the subject of heat, have occasionally added certain terms to distinguish their conceptions, such as, latent, absolute, specific, sensible heat; while others have adopted the new expressions of caloric, and the matter of heat. None of these descriptive appellations however would have completely answered my purpose. I might, as in the preceding papers, have used the name radiant heat, which has been introduced by a celebrated author, and which certainly is not very different from the expressions I have now adopted; but, by calling the subject of my researches, the rays that occasion heat, I cannot be misunderstood as meaning that these rays themselves are heat; nor do I in any respect engage myself to shew in what manner they produce heat.

From what has been said it follows, that any objections that may be alleged, from the supposed agency of heat in other

circumstances than in its state of radiance, or heat-making rays, cannot be admitted against my experiments. For, notwithstanding I may be inclined to believe that all phænomena in which heat is concerned, such as the expansion of bodies, fluidity, congelation, fermentation, friction, &c. as well as heat in its various states of being latent, specific, absolute, or sensible, may be explained on the principle of heat-making rays, and vibrations occasioned by them in the parts of bodies; yet this is not intended, at present, to be any part of what I shall endeavour to establish.

I must also remark, that in using the word rays, I do not mean to oppose, much less to countenance, the opinion of those philosophers who still believe that light itself comes to us from the sun, not by rays, but by the supposed vibrations of an elastic ether, every where diffused throughout space; I only claim the same privilege for the rays that occasion heat, which they are willing to allow to those that illuminate objects. For, in what manner soever this radiance may be effected, it will be fully proved hereafter, that the evidence, either for rays, or for vibrations which occasion heat, stands on the same foundation on which the radiance of the illuminating principle, light, is built.

In order to enter on our subject with some regularity, it will be necessary to distinguish heat into six different kinds, three whereof are solar, and three terrestrial; but, as the divisions of terrestrial heat strictly resemble those of solar, it will not be necessary to treat of them separately; our subject, therefore, may be reduced to the three following general heads.

We shall begin with the heat of luminous bodies in general, such as, in the first place, we have it directly from the sun;

and as, in the second, we may obtain it from terrestrial flames, such as torches, candles, lamps, blue-lights, &c.

Our next division comprehends the heat of coloured radiants. This we obtain, in the first place, from the sun, by separating its rays in a prism; and, in the second, by having recourse to culinary fires, openly exposed.

The third division relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This, as I have shewn, is to be had, in the first place, directly from the sun, by means of a prism applied to its rays; and, in the second, we may have it from fires inclosed in stoves, and from red-hot iron cooled till it can no longer be seen in the dark.

Besides the arrangement in the order of my experiments which would arise from this division, we have another subject to consider. For, since the chief design of this paper is to give a comparative view of the operations that may be performed on the rays that occasion heat, and of those which we already know to have been effected on the rays that occasion light, it will be necessary to take a short review of the latter. I shall merely select such facts as not only are perfectly well known, but especially such as will answer the intention of my comparative view, and arrange them in the following order.

1. Light, both solar and terrestrial, is a sensation occasioned by rays emanating from luminous bodies, which have a power of illuminating objects; and, according to circumstances, of making them appear of various colours.

2. These rays are subject to the laws of reflection.

3. They are likewise subject to the laws of refraction.

4. They are of different refrangibility.

5. They are liable to be stopped, in certain proportions, when transmitted through diaphanous bodies.

6. They are liable to be scattered on rough surfaces.

7. They have hitherto been supposed to have a power of heating bodies; but this remains to be examined.

The similar propositions relating to heat, which are intended to be proved in this paper, will stand as follows.

1. Heat, both solar and terrestrial, is a sensation occasioned by rays emanating from candent substances, which have a power of heating bodies.

2. These rays are subject to the laws of reflection.

3. They are likewise subject to the laws of refraction.

4. They are of different refrangibility.

5. They are liable to be stopped, in certain proportions, when transmitted through diaphanous bodies.

6. They are liable to be scattered on rough surfaces.

7. They may be supposed, when in a certain state of energy, to have a power of illuminating objects; but this remains to be examined.

Before I can go on, I have to mention, that the number of experiments which will be required to make good all these points, exceeds the usual length of my papers; on which account, I shall divide the present one into two parts. Proceeding therefore now to an investigation of the three first heads that have been proposed, I reserve the three next, and a discussion which will be brought on by the seventh article, for the second part.

1st. Experiment. Reflection of the Heat of the Sun.

I exposed the thermometer which in a former paper has been denoted by No. 3, to the eye-end of a ten-feet NEWTONIAN

telescope, which carried a *Camera-eye-piece*,* but no eye-glass. When, by proper adjustment, the focus came to the ball of the thermometer, it rose from 52 degrees to 110; so that rays which came from the sun, underwent three regular reflections; one, on a concave mirror, and the other two, on two plain ones. Now these rays, whether they were those of light or not, for that our experiment cannot ascertain, had a power of occasioning heat, which was manifested in raising the thermometer 58 degrees.

2d. Experiment. Reflection of the Heat of a Candle.

At the distance of 29 inches from a candle, I planted a small steel-mirror, of $3\frac{4}{10}$ inches diameter, and about $2\frac{1}{4}$ inches focal length.† In the secondary focus of it, I placed the ball of the thermometer which in my paper has been marked No. 2; and very near it, but out of the reach of reflection, the thermometer No. 3. Having covered the mirror till both were come to the temperature of their stations, I began as follows.

	No. 2. In the Focus.	No. 3. Standard.
o′	54	54
1	55	54
2	56	54
3	57	54
4	$57\frac{1}{4}$	54
5	$57\frac{1}{4}$	54

Here, in five minutes, the thermometer No. 2 received $3\frac{1}{4}$ degrees of heat from the candle, by reflected rays. I now

* See Phil. Trans. Vol. LXXII. p. 176.

† See Plate XII. Fig. 1.

covered the mirror, but left all the rest of the apparatus untouched.

	No. 2. In the Focus.	No. 3. Standard.
0′	$57\frac{1}{4}$	54
1	$55\frac{1}{2}$	54
$1\frac{1}{2}$	55	54
6	54	54

Here, in six minutes, the thermometer lost the $3\frac{1}{4}$ degrees of heat again, which it had gained before. I uncovered the mirror once more.

0′	54	54
$1\frac{1}{2}$	56	54
$3\frac{1}{2}$	57	54
5	$57\frac{1}{4}$	54

And, in five minutes, the $3\frac{1}{4}$ degrees of heat were regained. In consequence of which, we are assured that certain rays came from the candle, subject to the laws of reflection, which, though they might not be the rays of light, for that our experiment does not determine, were evidently invested with a power of heating the thermometer placed in the focus of the mirror.

3d Experiment. Reflection of the Heat that accompanies the Solar prismatic Colours.

In the spectrum of the sun, given by a prism, I placed my small steel mirror, with a thermometer in its focus.* It was covered by a piece of pasteboard, which, through a proper opening, admitted all the visible colours to fall on its polished surface, but excluded every other ray of heat that might be, either on the violet or on the red side, beyond the spectrum.

* See Plate XII. Fig. 2.

Then, placing the apparatus so as to have the thermometer in the red rays, but keeping the mirror covered up till the thermometer became settled, I found it stationary at 58°. Uncovering the mirror, I had as follows.

	No. 2.
0′	58
2	93

Here, in two minutes, the thermometer rose 35 degrees, by reflected heat. I covered the mirror again, and, in a few minutes, the thermometer, exposed to the direct prismatic red, came down to 58° again. And thus the prismatic colours, if they are not themselves the heat-making rays, are at least accompanied by such as have a power of occasioning heat, and are liable to be regularly reflected.

4th Experiment. Reflection of the Heat of a red-hot Poker.

I placed the small steel mirror at 12 inches from a red-hot poker, set with its heated end upwards, in a perpendicular position, and so elevated as to throw its rays on the mirror.* The thermometer No. 2 was placed in its secondary focus, and had a small pasteboard screen, to guard its ball from the direct heat of the poker.

	No. 2.
0′	$54\frac{1}{2}$
$1\frac{1}{2}$	93

I covered the mirror.

3′	65

Here, in $1\frac{1}{2}$ minute, the thermometer rose $38\frac{1}{2}$ degrees, by reflected rays; and, when the mirror was covered up, it fell in the next $1\frac{1}{2}$ minute, 28 degrees. On which account, we cannot

* See Plate XII. Fig. 1.

but allow, that certain rays, whether it be those that shine or not, issue from an ignited poker, which are subject to the regular laws of reflection, and have a power of heating bodies.

5th Experiment. Reflection of the Heat of a Coal Fire by a plain Mirror.

I placed a small speculum, such as I use with my 7-feet reflectors, upon a stand, and so as to make an angle of 45 degrees with the front of it.* This was afterwards to face the fire in my parlour chimney, and would make the same angle with the bars of the grate. At a distance of $3\frac{1}{2}$ inches from the speculum, on the reflecting side of it, was placed the thermometer No. 1; and close by it, but out of the reach of the reflected rays, the thermometer No. 4. The whole was guarded in front, against the influence of the fire, by an oaken board $1\frac{1}{2}$ inch thick, which had a circular opening of $1\frac{1}{4}$ inch diameter, opposite the situation of the plain mirror, in order to permit the fire to shine upon it. The thermometers were divided from the mirror by a wooden partition, which also had an opening in it, that the reflected rays might come from the mirror to No. 1, while No. 4 remained screened from their influence. On exposing this apparatus to the fire, I had the following result.

	No. 1.	No. 4.
0′	60	60
1	62	60
2	64	60
3	66	60
4	66	60
5	67	$60\frac{1}{2}$

* See Plate XII. Fig. 3.

Here, in five minutes, the heat reflected from the plain mirror raised the thermometer No. 1, 7 degrees; while the change in the temperature of the screened place, indicated by No. 4, amounted only to half a degree: which shews, that an open fire sends out rays that are subject to the laws of reflection, and occasion heat.

6th Experiment. Reflection of Fire-heat by a Prism.

Every thing remaining arranged as in the 5th experiment, I removed the small plain mirror, and placed in its stead a prism, which had one of its angles of 90 degrees, and the other two of 45° each.* It was put so as to have one of the sides facing the fire, while the other was turned towards the thermometer: the hypotenuse, consequently, made an angle of 45 degrees with the bars of the grate. The apparatus, after having been cooled some time, was exposed to the fire, and the following result was taken.

	No. 1.	No. 4.
0′	62½	62½
1	63	62¾
2	64	63
4	64½	63
5	65	63¼
8	65¾	63½
10	66½	63¾
11	67	64¼

Here, in eleven minutes, the rays reflected by the prism raised the thermometer 4½ degrees; but, the temperature of the place having undergone an alteration of 1¼ degrees, we can only

* See Plate XII. Fig. 3.

place $2\frac{1}{4}$ to the account of reflection. The apparatus becoming now very hot, it would not have been fair to have continued the experiment for a longer time; but the effect already produced was fully sufficient to shew, that even a prism, which stops a great many heat-making rays, still reflects enough of them to prove, that an open fire not only sends them out, but that they are subject to every law of reflection.

7th Experiment. Reflection of invisible Solar Heat.

On a board of about 4 feet 6 inches long, I placed at one end, a small plain mirror, and at the other, two thermometers*. The distance of No. 1, from the face of the mirror, was 3 feet $9\frac{1}{4}$ inches; and No. 2 was put at the side of it, facing the same way, but out of the reach of the rays that were to be reflected by the mirror. The colours of the prism were thrown on a sheet of paper having parallel lines drawn upon it, at half an inch from each other. The mirror was stationed upon the paper; and was adjusted in such a manner as to present its polished surface, in an angle of 45 degrees, to the incident coloured rays, by which means, they would be reflected towards the ball of the thermometer No. 1. In this arrangement, the whole apparatus might be withdrawn from the colours to any required distance, by attending to the last visible red colour, as it shewed itself on the lines of the paper. When the thermometers were properly settled to the temperature of their situation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours, so far as to cause the mirror, which was now open, to receive only the invisible rays of heat which lie beyond the confines of red. The result was as follows.

* See Plate XII. Fig. 4.

	No. 1.	No. 2.
0′	56	56
—	57	56
—	59	56
7	60	56
10	60	56

Here, in ten minutes, the thermometer No. 1 received four degrees of heat, reflected to it, in the strictest optical manner, by the plain mirror of a NEWTONIAN telescope. The great regularity with which these invisible rays obeyed the law of reflection, was such, that Dr. WILSON's sensible thermometer No. 2, which had been chosen on purpose for a standard, and was within an inch of the other thermometer, remained all the time without the least indication of any change of temperature that might have arisen from straggling rays, had there been any such. I now took away the mirror, but left every thing else in the situation it was. The effect of this was thus.

	No. 1.	No. 2.
0′	60	56
5	58	56
8	57	56
10	56	56

Here, in ten minutes, the thermometer No. 1 lost again the 4 degrees it had acquired, while No. 2 still remained unaltered; and this becomes therefore a most decisive experiment, in proof of the existence of invisible rays, of their being subject to the laws of reflection, and of their power of occasioning heat.

8th Experiment. Reflection, and Condensation, of the invisible solar Rays.

I made an apparatus for placing the small steel mirror at any required angle;* and, having exposed it to the prismatic spectrum, so as to receive it perpendicularly, I caused the colours to fall on one half of the mirror, which, being covered by a semicircular piece of pasteboard, would stop all visible rays, so that none of them could reach the polished surface. On the pasteboard were drawn several lines, parallel to the diameter, and at the distance of one-tenth of an inch from each other; that, by withdrawing the apparatus, I might have it at option to remove the last visible red to any required distance from the reflecting surface. In the focus of the mirror was placed the thermometer No. 2. I covered now also the other half of the mirror, till the thermometer had assumed the temperature of its situation. Then, withdrawing the apparatus out of the visible spectrum, till the last tinge of red was one-tenth of an inch removed from the edge of the pasteboard, and the whole of the coloured image thus thrown on the semicircular cover, I opened the other half of the mirror, for the admission of invisible rays. The result was as follows.

No. 2.

In the Focus of invisible Heat.

0′	61
1	80

Here, in one minute, the thermometer rose 19 degrees. I covered the mirror.

2′	72
3	67
4	64

* See Plate XII. Fig. 2.

Here, in three minutes, the thermometer fell 16 degrees. I opened the mirror again.

No. 2.

In the Focus of invisible Heat.

5′	83
6	88

Here, in two minutes, the thermometer rose 24 degrees. I covered the mirror once more.

7′	69

And, in one minute, the thermometer fell 19 degrees. Now, by this alternate rising and falling of the thermometer, three points are clearly ascertained. The first is, that there are invisible rays of the sun. The second, that these rays are not only reflexible, in the manner which has been proved in the foregoing experiment, but that, by the strict laws of reflection, they are capable of being condensed. And, in the third place, that by condensation, their heating power is proportionally increased; for, under the circumstances of the experiment, we find that it extended so far as to be able to raise the thermometer, in two minutes, no less than 24 degrees.

9th Experiment. Reflection of invisible culinary Heat.

I planted my little steel mirror upon a small board;* and at a proper distance opposite to it I erected a slip of deal, $\frac{1}{2}$ inch thick, and 1 inch broad, in a horizontal direction, so as to be of an equal height, in the middle of its thickness, with the centre of the mirror. Against the side, facing the mirror, were fixed the two thermometers No. 2 and No. 3, with their balls within half an inch of each other, and the scales turned the opposite way. A little of the wood was cut out of the slip, to

* See Plate XIII. Fig. 1.

make room for the balls to be freely exposed. That of No. 2 was in the axis of the mirror; and the ball of No. 3 was screened from the reflected rays, by a small piece of pasteboard tied to the scale. The small ivory scales of the thermometers, with the slip of wood at their back, which however was feather-edged towards the stove, intercepted some heat; but it will be seen presently that there was enough to spare. When my stove was of a good heat, I brought the apparatus to a place ready prepared for it.

	No. 2. In the Focus.	No. 3. Screened.
0′	52	52
1	91	53

Here we find that, in one minute, the invisible culinary heat raised the thermometer No. 2, 39 degrees; while No. 3, from change of temperature, rose only one, notwithstanding its exposure to the stove was in every respect equal to that of No. 3, except so far as relates to the rays returned by the mirror; and therefore, the radiant nature of these invisible rays, their power of heating bodies, and their being subject to the laws of reflection, are equally established by this experiment.

10*th Experiment. Reflection of the invisible Rays of Heat of a Poker, cooled from being red-hot till it could no longer be seen in a dark Place.*

The great abundance of heat in my last experiment, would not allow of its being carried on without injury to the thermometer, the scale of which is not extensive; I therefore placed a poker, when of a proper black heat, at 12 inches from the steel mirror,* and received the effect of its condensed rays upon the

* See Plate XII. Fig. 1.

thermometer No. 2, placed in the focus. Then, alternately covering and uncovering the mirror, one minute at a time, the effect was as follows.

		No. 2.
The mirror covered	0′	61
Open - -	1	68
Covered -	2	61
Open - -	3	64
Covered -	4	59
Open - -	5	61½
Covered -	6	58

Here, in six minutes, we have a repeated result of alternate elevations and depressions of the thermometer, all of which confirm the reflexibility, the radiant nature, and the heating power, of the invisible rays that came from the poker.

From these experiments it is now sufficiently evident, that in every supposed case of solar and terrestrial heat, we have traced out rays that are subject to the regular laws of reflection, and are invested with a power of heating bodies; and this independent of light. For though, in four cases out of six, we had illuminating as well as heating rays, it is to be noticed that our proof goes only to the power of occasioning heat, which has been strictly ascertained by the thermometer. If it should be said, that the power of illuminating objects, of these same rays, is as strictly proved by the same experiments, I must remark, that from the cases of invisible rays brought forward in the four last experiments, it is evident that the conclusion, that rays must have illuminating power, because they have a power of occasioning heat, is erroneous; and, as this must be admitted, we have a right to ask for some proof of the assertion, that rays

which occasion heat can ever become visible. But, as we shall have an opportunity to say more of this hereafter, I proceed now to investigate the refraction of heat-making rays.

11th Experiment. Refraction of solar Heat.

With a new ten-feet NEWTONIAN telescope, the mirror of which is 24 inches in diameter of polished surface, I received the rays of the sun; and, making them pass through a day-piece with four lenses, I caused them to fall on the ball of the thermometer No. 3, placed in their focus. Those who are acquainted with the lines in which the principal rays and pencils move through a set of glasses, will easily conceive how artfully, in our present instance, heat was sent from one place to another. Heat crossing heat, through many intersecting courses, without jostling together, and each parcel arriving at last safely to its destined place. As soon as the rays were brought to the thermometer, it rose almost instantly from 60 degrees to 190; and, being afraid of cracking the glasses, I turned away the telescope. Here the rays, which occasioned no less than 70 degrees of heat, had undergone eight regular successive refractions; so that their being subject to its laws cannot be doubted.

12th Experiment. Refraction of the Heat of a Candle.

I placed a lens of about 1,4 inch focus, and 1,1 diameter, mounted upon a small support, at a distance of 2,8 inches from a candle;* and the thermometer No. 2, behind the lens, at an equal distance of about 2,8 inches; but which ought to be very carefully adjusted to the secondary focus of the candle. Not far from the lens, towards the candle, was a pasteboard screen, with an aperture of nearly the same size as the lens. The sup-

* See Plate XIII. Fig. 2.

port of the lens had an eccentric pivot, on which it might be turned away from its place, and returned to the same situation again, at pleasure. This arrangement being made, the thermometer was for a few moments exposed to the rays of the candle, till it had assumed the temperature of its situation. Then the lens was turned on its pivot, so as to intercept the direct rays, which passed through the opening in the pasteboard screen, and to refract them to the focus, in which the thermometer was situated.

No. 2.

0′	$53\frac{7}{8}$
1	$55\frac{1}{2}$
2	$55\frac{3}{4}$
3	56

Here, in three minutes, the thermometer received $2\frac{1}{8}$ degrees of heat, by the refraction of the lens. The lens was now turned away.

0′	56
1	$54\frac{5}{8}$
2	$54\frac{1}{8}$
3	$53\frac{7}{8}$

Here, in three minutes, the thermometer lost $2\frac{1}{8}$ degrees of heat. The lens was now returned to its situation.

0′	$53\frac{7}{8}$
1	$54\frac{3}{4}$
2	$55\frac{3}{8}$
3	56

And, in three minutes, the thermometer regained the $2\frac{1}{8}$ degrees of heat. A greater effect may be obtained by a different

arrangement of the distances. Thus, if the lens be placed at $9\frac{1}{4}$ inches from a wax-candle, and the thermometer situated, as before, in the secondary focus, we shall be able to draw from 5 to 8 degrees of heat, according to the burning of the candle, and the accuracy of the adjustment of the thermometer to the focus. The experiment we have related shews evidently, that rays invested with a power of heating bodies, issue from a candle, and are subject to laws of refraction, nearly the same with those respecting light.

13th Experiment. Refraction of the Heat that accompanies the coloured part of the prismatic Spectrum.

I covered a burning lens of Mr. DOLLOND's, which is nearly 9 inches in diameter, and very highly polished, with a piece of pasteboard, in which there was an opening of a sufficient size to admit all the coloured part of the prismatic spectrum.* In the focus of the glass was placed the thermometer No. 3; and, when every thing was arranged properly, I covered the lens for five minutes, that the thermometer might assume the temperature of its situation. The result was as follows.

		No. 3.
The lens covered	0′	64
Open - -	1	176

Here, in one minute, the thermometer received 112 degrees of heat, which came with the coloured part of the solar spectrum, and were refracted to a focus; so that, if the coloured rays themselves are not of a heat-making nature, they are at least accompanied with rays that have a power of heating bodies, and

* See Plate XIV.

are subject to certain laws of refraction, which cannot differ much from those affecting light.

14th Experiment. Refraction of the Heat of a Chimney-Fire.

I placed Mr. DOLLOND's lens before the clear fire of a large grate.* Its distance from the bars of the grate was three feet; and, in the secondary focus of it was placed the thermometer No. 1. No. 4 was stationed, by way of standard, at $2\frac{1}{2}$ inches from the former, and at an equal distance from the fire. Before the thermometers was a slip of mahogany, which had three holes in it, $\frac{1}{10}$ of an inch in diameter each. Behind the centre of the 1st hole, $\frac{1}{8}$ of an inch from the back, was placed the thermometer No. 1; and, between the 2d and 3d hole, guarded from the direct rays of the fire by the partition, at the same distance from the back, was put No. 4. Things being thus arranged, the situation of the apparatus which carried the thermometers, and that where the lens was fixed, were marked. Then the thermometers, having been taken away to be cooled, were restored to their places again, and their progress marked as follows.

	No. 1. Burning Lens.	No. 4. Screened.
0′	58	58
$1\frac{1}{2}$	65	60
3	68	61
5	70	$61\frac{1}{2}$
7	$71\frac{1}{4}$	$61\frac{3}{4}$
9	$71\frac{1}{2}$	$61\frac{3}{4}$

Here, in nine minutes, the rays coming from the fire, through the burning glass, gave $9\frac{1}{4}$ degrees of heat more to the ther-

* See Plate XV.

mometer No. 1, than No. 4, from change of temperature, had received behind the screen. Now, to determine whether this was owing merely to a transmission of heat through the glass, or to a condensation of the rays, by the refraction of the burning lens, I took away the lens, as soon as the last observation of the thermometers was written down, and continued to take down their progress as follows.

	No. 1.	No. 4.
$9'\frac{1}{2}$	$71\frac{1}{2}$	$61\frac{3}{4}$
11	$70\frac{1}{2}$	$61\frac{3}{4}$
12	$70\frac{1}{4}$	$61\frac{3}{4}$
—	$69\frac{1}{2}$	$61\frac{3}{4}$
$14\frac{1}{2}$	$69\frac{1}{4}$	$61\frac{3}{4}$

Here the direct rays of the fire, we see, could not keep up the thermometer No. 1; which lost $2\frac{1}{4}$ degrees of heat, notwithstanding the lens intercepted no longer any of them. I now restored the burning glass, and continued.

15′	$69\frac{1}{4}$	$61\frac{1}{4}$
16	$69\frac{1}{2}$	$61\frac{3}{4}$
17	70	$61\frac{1}{4}$
20	$70\frac{3}{4}$	$61\frac{3}{4}$
25	71	$61\frac{3}{4}$

Here again, the lens acted as a condenser of heat, and gave $1\frac{3}{4}$ degrees of it to the thermometer No. 1. I now once more took away the lens, and continued the experiment.

$25'\frac{1}{2}$	71	$61\frac{3}{4}$
31	68	$61\frac{3}{4}$

This again confirms the same, by a loss of 3 degrees of heat. I restored the lens once more, and had as follows.

	No. 1. Burning Lens.	No. 4. Screened.
31′½	68	61¼
35	69½	61¼

And here the thermometer received 1½ degree of heat again; so that, in the course of 35 minutes, the thermometer No. 1 was alternately raised and depressed five times, by rays which came from the chimney fire, and were subject to laws of refraction, not sensibly different from those which affect light.

15th Experiment. Refraction of the Heat of red-hot Iron.

I caused a lump of iron to be forged into a cylinder of 2½ inches diameter, and 2½ inches long.* This, being made red-hot, was stuck upon an iron handle fixed on a stand, so as to present one of its circular faces to a lens placed at 2,8 inches distance; its focus being 1,4 inch, and diameter 1,1. Before the lens, at some distance, was placed a screen of wood, with a hole of an inch diameter in it, by way of limiting the object, that its image in the focus might not be larger than necessary. The screen also served to keep the heat from the thermometers. No. 2 was situated in the secondary focus of the lens; and No. 3 was placed within $\frac{3}{10}$ of an inch of it, and at the same distance from the lens as No. 2. By this arrangement, both thermometers were equally within the reach of transmitted heat; or, if there was any difference, it could only be in favour of No. 3, as being behind a part of the lens which, on account of its thinness, would stop less heat than the middle. Now, as the experiment gives a result which differs from what would have arisen from the situation of the thermometers, on a supposition of transmitted heat, we can only ascribe it to a conden-

* See Plate XVI. Fig. 1.

sation of it by the refraction of the lens; and, in this case, the thermometer No. 3, by its situation, must have been partly within the reach of the heat-image formed in the focus. During the experiment, the thermometers were alternately screened two minutes from the effects of the lens, and exposed to it for the same length of time; and the result was as follows.

		No. 2 In the Focus.	No. 3. Near the Focus.
Screened	0′	56	56
Open	2	62	60
Screened	4	59	58
Open	6	61	59
Screened	8	$58\frac{1}{4}$	$57\frac{3}{4}$
Open	10	$59\frac{1}{2}$	$58\frac{1}{4}$

Here, in the first and second minutes, No. 2 gained two degrees of heat more than No. 3. In the third and fourth, it lost one more than No. 3. In the fifth and sixth, it gained one more. In the seventh and eighth, it lost $1\frac{1}{2}$ more; and in the ninth and tenth, it gained $\frac{3}{4}$ more than the other thermometer. This plainly indicates its being acted upon by refracted heat. Lest there should remain a doubt upon the subject, I now removed the lens, and, putting a plain glass in the room of it, I repeated the experiment, with all the rest of the apparatus in its former situation.

Screened	0′	$57\frac{1}{4}$	$56\frac{3}{4}$
Open	2	$62\frac{1}{4}$	$61\frac{3}{4}$
Screened	4	$60\frac{1}{2}$	60
Open	6	61	$60\frac{1}{2}$
Screened	8	60	$59\frac{1}{2}$
Open	10	$60\frac{4}{3}$	$60\frac{1}{4}$

Here we find, that both thermometers received heat and parted with it always in equal quantities, which confirms the experiment that has been given. And thus it is evident, that there are rays issuing from red-hot iron, which are subject to laws of refraction, nearly equal to those which affect light; and that these rays are invested with a power of causing heat in bodies.

16*th Experiment. Refraction of Fire-heat, by an Instrument resembling a Telescope.*

It occurred to me, that I might use a concave mirror, to condense the heat of the fire in the grate of my chimney, and, reflecting it sideways by a plain mirror, I might afterwards bring it to a secondary focus by a double convex lens; and that, by this construction, I should have an instrument much like a NEWTONIAN telescope.* The thermometer would figuratively become the observer of heat, by being applied to the place where, in the real telescope of the same construction, the eye is situated to receive light. Having put together the different parts, in such a way as I supposed would answer the end, I tried the effect by a candle, in order to ascertain the proper distance of the object-mirror from the bars of the chimney-grate. The front of the apparatus was guarded by an iron plate, with a thick lining of wood; and the two thermometers which I used, were parted from the mirrors and lens by a partition, which screened them from the heat that was to be admitted through a proper opening in the front plate, to come at the object-mirror. In the partition was likewise an opening, of a sufficient diameter to permit the rays to come from the eye-glass to their focus, on the ball of the thermometer No. 1; while

* See Plate XVI. Fig. 2.

No. 4 was placed by the side of it, at less than half an inch distance. In the experiment, the object-mirror was alternately covered by a piece of pasteboard, and opened again. The thermometers were read off every minute; but, to shorten my account, I only give the last minute of every change.

		No. 1. In the Focus.	No. 4. Near the Focus.
The mirror covered	0′	77½	77½
The mirror open	8	84	76
Covered - -	16	86½	79½
Open -	21	89¾	81
Covered -	27	89¾	82¼
Open - -	37	91½	83½
Covered -	47	84	77

Here, in the first eight minutes, the thermometer exposed to the effects of the fire-instrument, gained 2 degrees of heat more than the other. In the next 8 minutes, the mirror being covered, it gained 1 degree less than the other. The mirror being now opened again, it gained, in 5 minutes, 2¼ degrees more than the other. When covered 6 minutes, it gained 1¼ degree less than No. 4. In the next 10 minutes, when open, it gained ½ degree more; and, in the last 10 minutes, when the fire began to fail, and the mirror was again covered, it lost 1 degree more than the other thermometer. All which can only be accounted for by the heat which came to the thermometer through the fire-instrument; and, as this experiment confirms what has been said before of the refraction of culinary heat, so it also adds to what has already been proved of its reflection. For, in this fire-instrument, the rays which occasion heat could undergo no less than two reflections and two refractions.

17th Experiment. Refraction of the invisible Rays of solar Heat.

I covered one half of Mr. DOLLOND's burning lens with pasteboard, and threw the prismatic spectrum upon that cover;* then, keeping the last visible red colour one-tenth of an inch from the margin of the pasteboard, I let the invisible rays beyond the spectrum fall on the lens. In the focus of the red rays, or a very little beyond it, I had placed the ball of the thermometer No. 1; and, as near to it as convenient, the small one No. 2. Now, that the invisible solar rays which occasion heat were accurately refracted to a focus, may be seen by the following account of the thermometers.

	No. 1. In the Focus.	No. 2. Near the Focus.
0′	57	57
1	102	57

Here, in one minute, these rays gave 45 degrees of heat to the thermometer No. 1, which received them in the focus, while the other, No. 2, suffered no change.

It is remarkable, that notwithstanding I kept the red colour of the spectrum $\frac{1}{10}$ of an inch upon the pasteboard, a little of that colour might still be seen on the ball of the thermometer. This occasioned a surmise, that possibly the invisible rays of the sun might become visible, if they were properly condensed; I therefore put this to the trial, as follows.

18th Experiment. Trial to render the invisible Rays of the Sun visible by Condensation.

Leaving the arrangement of my apparatus as in the last experiment, I withdrew the lens, till the last visible red colour

* See Plate XIV.

was two-tenths of an inch from the margin of the semicircular pasteboard cover; then, taking the thermometers, I had as follows.

	No. 2.	No. 3.
0′	57	57
1	78	57

Here, there was no longer the least tinge of any colour, or vestige of light, to be seen on the ball of the thermometer; so that, in one minute, it received 21 degrees of heat, from rays that neither were visible before, nor could be rendered so by condensation.

To account for the colour which may be seen in the focus, when the last visible red colour is less than two-tenths of an inch from the margin of the pasteboard which intercepts the prismatic spectrum, we may suppose, that the imperfect refraction of a burning lens, which from its great diameter cannot bring rays to a geometrical focus, will bring some scattered ones to it, which ought not to come there. We may also admit, that the termination of a prismatic spectrum cannot be accurately ascertained, by looking at it in a room not sufficiently dark to make very faint tinges of colour visible. And, to this must be added, that the incipient red rays must actually be scattered over a considerable space, near the confines of the spectrum, on account of the breadth of the prism, the whole of which cannot bring its rays of any one colour properly together; nor can it separate the invisible rays intirely from the visible ones. For, as the red rays will be but faintly scattered in the beginning of the visible spectrum, so, on the other hand, will the invisible rays, separated by the parts of the prism that come next in succession, be mixed with the former red ones. Sir ISAAC

NEWTON has taken notice of some imperfect tinges or haziness, on each side of the prismatic spectrum, and mentions that he did not take them into his measures.*

19th Experiment. Refraction of invisible culinary Heat.

There are some difficulties in this experiment; but they arise not so much from the nature of this kind of heat, as from our method of obtaining it in a detached state. A red-hot lump of iron, when cooled so far as to be no longer visible, has but a feeble stock of heat remaining, and loses it very fast. A contrivance to renew and keep this heat might certainly be made, and I should indeed have attempted to carry some method or other of this kind into execution, had not the following trials appeared to me sufficiently conclusive to render it unnecessary. Admitting, as has been proved in the 15th experiment, that the alternate rising and falling of a thermometer placed in the focus of a lens, when the ball of it is successively exposed to, or screened from, its effects, is owing to the refraction of the lens, and cannot be ascribed to a mere alternate transmission and stoppage of heat, I proceeded as follows.† My lens, 1,4 focus, and 1,1 diameter, being placed 2,8 inches from the face of the heated cylinder of iron, the thermometer No. 2, in its focus, was alternately guarded by a small pasteboard screen put before it, and exposed to the effects of condensed heat by removing it.

		No. 2.	
Screened	0′	55	Very red-hot.
Open	2	63½	Red-hot.
Screened	4	58	Still red-hot.
Open	6	60½	Still red.

* NEWTON's Optics, page 23, line 11. † See Plate XVI. Fig. 1.

No. 2.

Screened	8′	$57\frac{1}{2}$	A little red.
Open	10	$59\frac{1}{4}$	Doubtful.
Screened	12	$57\frac{1}{2}$	Not visible in my room darkened.
Open	14	$58\frac{1}{2}$	
Screened	16	$57\frac{1}{2}$	
Open	18	$58\frac{1}{4}$	
Screened	20	$57\frac{1}{4}$	
Open	22	58	
Screened	24	$57\frac{1}{2}$	
Open	26	58	
Screened	28	$57\frac{1}{2}$	

Now, the beginning of this experiment being exactly like that of the 15th, with the thermometer No. 3 left out, the arguments that have before proved the refraction of heat in one state, will now hold good for the whole. For here we have a regular alternate rising and falling of the thermometer, from a bright red heat of the cylinder, down to its weakest state of black heat; where the effect of the rays, condensed by the lens, exceeded but half a degree the loss of those that were stopped by it.

20th Experiment. Confirmation of the 19th.

In order to have some additional proof, besides the uniform and uninterrupted operation of the lens in the foregoing experiment, I repeated the same, with an assistant thermometer, No. 3, placed first of all at $\frac{1}{4}$ of an inch from No. 2, and more towards the lens, but so as to be out of the converging pencil of its rays, and also to allow room for the little screen between the two thermometers, that No. 3 might not be covered by it.

		No. 2. In the Focus.	No. 3. Advanced sideways. Always open.
Screened	0′	$62\frac{1}{2}$	63
Open	1	$63\frac{3}{4}$	64
Screened	2	$62\frac{7}{8}$	64
Open	3	64	$64\frac{1}{2}$
Screened	4	$63\frac{3}{4}$	$64\frac{1}{2}$
Open	5	$64\frac{7}{8}$	$64\frac{1}{2}$
Screened	6	$64\frac{1}{2}$	$64\frac{1}{2}$
Open	7	$64\frac{1}{4}$	64
Screened	8	$64\frac{1}{2}$	64

Here No. 3, being out of the reach of refraction, gradually acquired its maximum of heat, in consequence of an uniform exposure to the influence of the hot cylinder; after which, it began to decline. No. 2, on the contrary, came to its maximum by alternate great elevations, and small depressions; and afterwards lost its heat by great depressions, and small elevations. After the first eight minutes, I changed the place of the assistant thermometer, by putting it into a still more decisive situation; for it was now placed by the side of that in the focus, so as to participate of the alternate screening, and also to receive a small share of one side of the invisible heat-image, which, though unseen, we know must be formed in the focus of the lens. Here, if our reasoning be right, the assistant thermometer should be affected by alternate risings and fallings; but they should not be so considerable as those of the lens.

		No. 2. In the Focus.	No. 3. In the Edge of it.
Both open	8′	$64\frac{1}{2}$	64
Both open	$9\frac{1}{2}$	$63\frac{3}{4}$	$63\frac{3}{4}$

		No. 2. In the Focus.	No. 3. In the Edge of it.
Open	11	$64\frac{1}{4}$	64
Screened	$12\frac{1}{2}$	63	$63\frac{1}{4}$
Open	14	$63\frac{3}{4}$	$63\frac{1}{2}$
Screened	16	$62\frac{3}{4}$	63
Open	18	$63\frac{3}{4}$	$63\frac{3}{4}$

Here the changes of the thermometer No. 2 were $-\frac{3}{4}+\frac{1}{2}-1\frac{1}{4}+\frac{3}{4}-1+1$; and those of No. 3 were $-\frac{1}{4}+\frac{1}{4}-\frac{3}{4}+\frac{1}{4}-\frac{1}{2}+\frac{3}{4}$. All which so clearly confirm the effect of the refraction of the lens, that it must now be evident that there are rays issuing from hot iron, which, though in a state of total invisibility, have a power of occasioning heat, and obey certain laws of refraction, very nearly the same with those that affect light.

As we have now traced the rays which occasion heat, both solar and terrestrial, through all the varieties that were mentioned in the beginning of this paper, and have shewn that, in every state, they are subject to the laws of reflection and of refraction, it will be easy to perceive that I have made good a proof of the three first of my propositions. For, the same experiments which have convinced us that, according to our second and third articles, heat is both reflexible and refrangible, establish also its radiant nature, and thus equally prove the first of them.

END OF THE FIRST PART.

Slough, near Windsor.
April 26, 1800.

EXPLANATION OF THE FIGURES.

See Plates XII. XIII. XIV. XV. and XVI.

Plate XII. Fig. 1.

Shews the arrangement of the apparatus used in the 2d experiment.

A, is the small mirror with its adjusting screws *m*, *n*.

No. 2, is the thermometer in the focus of the mirror.

No. 3, The assistant thermometer.

B, A small screen for the thermometer No. 2.

C, The candle.

D, The poker which, in the 4th and 10th experiments, is to be placed in the situation of the candle; the rest of the apparatus being brought nearer to it.

Plate XII. Fig. 2.

Shews the apparatus used in the 3d and 8th experiments.

A, The mirror.

No. 2, The thermometer.

BCD, A desk adjustable to different altitudes.

E, The prism receiving the sun's rays through an opening in the window shutter F.

Plate XII. Fig. 3.

AB, is the front of the apparatus, which, in the 5th experiment, is exposed to the fire of the chimney.

C, is the opening in the front plate AB, for the admission of heat.

D, is the small mirror which reflects the rays of heat.

E, is the hole through which the heat passes to the thermometers.

No. 1 and No. 4, are the thermometers.

F, is a prism, which, in the 6th experiment, is to be placed in the room of the mirror D.

Plate XII. Fig. 4.

A, is the board that holds the apparatus used in the 7th experiment.

B, The prism.

C, The spectrum, thrown partly on the paper with parallel lines, and partly on one of the small tables which support the board.

D, The mirror which reflects the rays of heat sideways.

No. 1, The thermometer which receives the reflected rays.

No. 2, The standard thermometer.

Plate XIII. Fig. 1.

AB, is the front which, in the 9th experiment, is put close to the flat side of a heated iron stove.

C, is the mirror.

D, The feather-edged slip of deal, on two pins.

No. 2, The thermometer which receives the rays condensed in the focus of the mirror.

No. 3, The standard thermometer.

E, A small screen tied to No. 3, to guard it from reflected heat.

Plate XIII. Fig. 2.

A, The lens in the apparatus used for the 12th experiment.

No. 2, The thermometer placed in its focus.

B, The screen with an aperture for admitting the rays of heat.

C, The eccentric pivot for turning away the lens.

D, The candle.

Plate XIV.

A, The burning lens, covered; with the prismatic spectrum thrown upon an opening, left for it, in the pasteboard cover of the 13th experiment.

No. 3, The thermometer placed in its focus.

B, The prism.

C, Semicircular cover, used in the 17th and 18th experiments, instead of the one with a square hole.

Plate XV.

A, The burning lens of the 14th experiment.

B, The fire in the chimney.

No. 1, The thermometer in the focus of the lens.

No. 4, The standard thermometer.

C, The hole through which the rays of heat pass to No. 1.

D and E, Two holes, between which the ball of the thermometer No. 4 is screened from the direct rays of the fire; while free access is given to the heat which may affect the temperature of the place.

Plate XVI. Fig. 1.

A, The iron cylinder, stuck upon its handle, as it is used in the 15th and 19th experiments.

B, The lens.

C, The screen with an opening in it.

No. 2, The thermometer in the focus of the lens.

No. 3, The standard thermometer.

D, The little moveable pasteboard screen.

Plate XVI. Fig. 2.

AB, The front, plated with iron, that it may bear to be exposed close to the bars of a chimney fire.

C, The concave mirror.

D, The plain mirror.

E, The lens.

No. 1, The thermometer in the focus of the lens.

No. 4, The standard thermometer.

F, A circular opening in the front plate AB, for admitting the rays of heat to fall on the concave mirror C.

m, The first focus of the rays, from which they go on diverging, to the small mirror, and to the lens; which brings them to a second focus, on the ball of the thermometer No. 1.

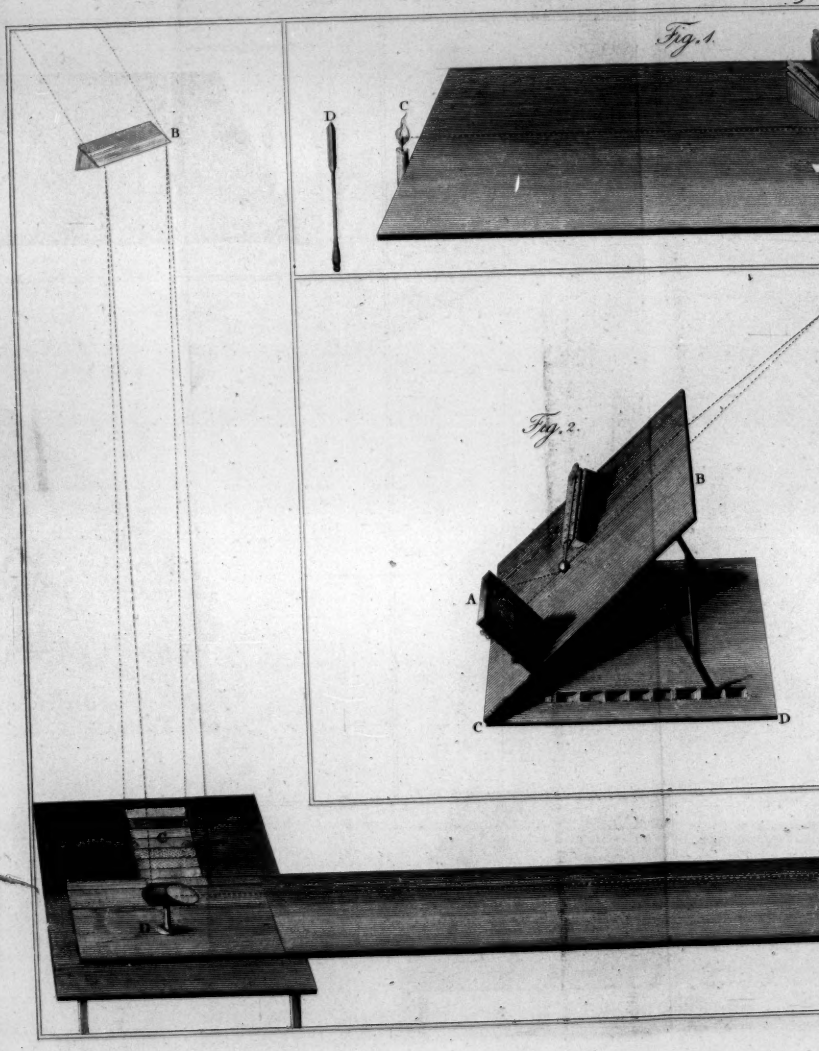
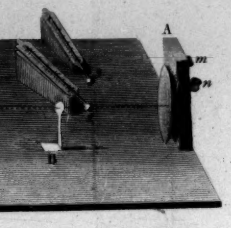


Fig. 1.
B
D
C
Fig. 2.
B
A
C
D
G
D



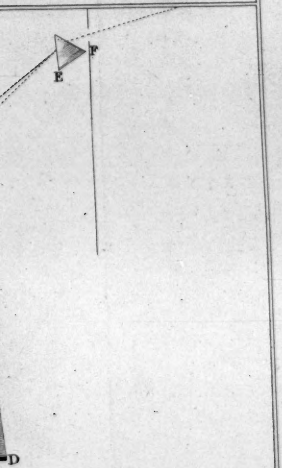


Fig. 3.

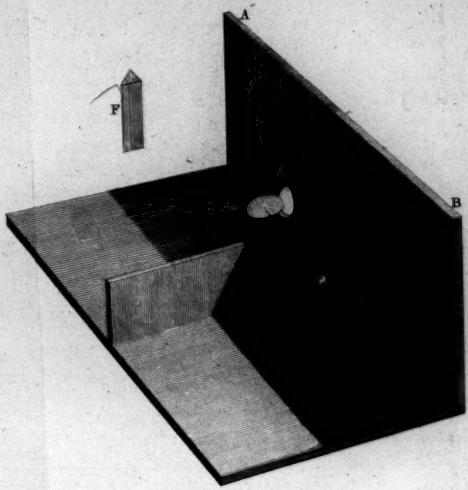
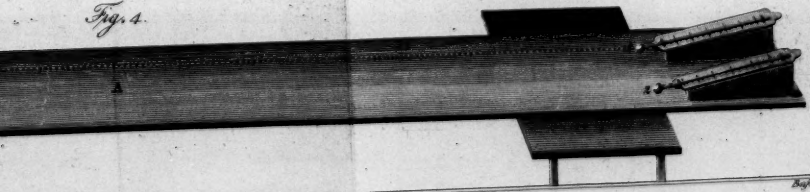


Fig. 4.



Basire fc

Fig. 1.

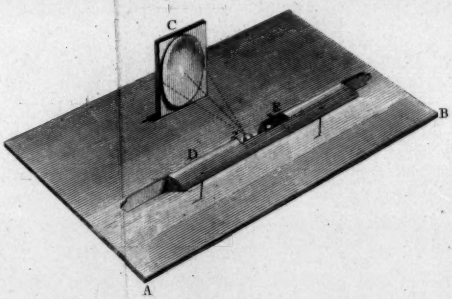
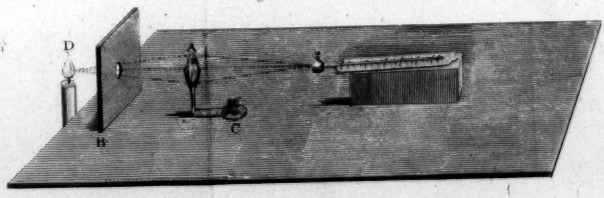


Fig. 2.



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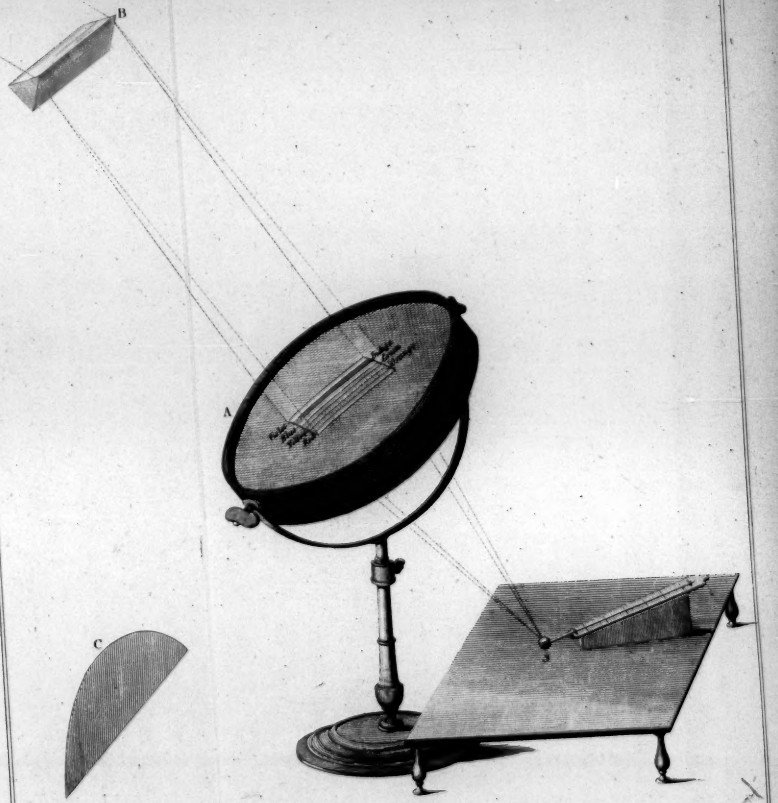

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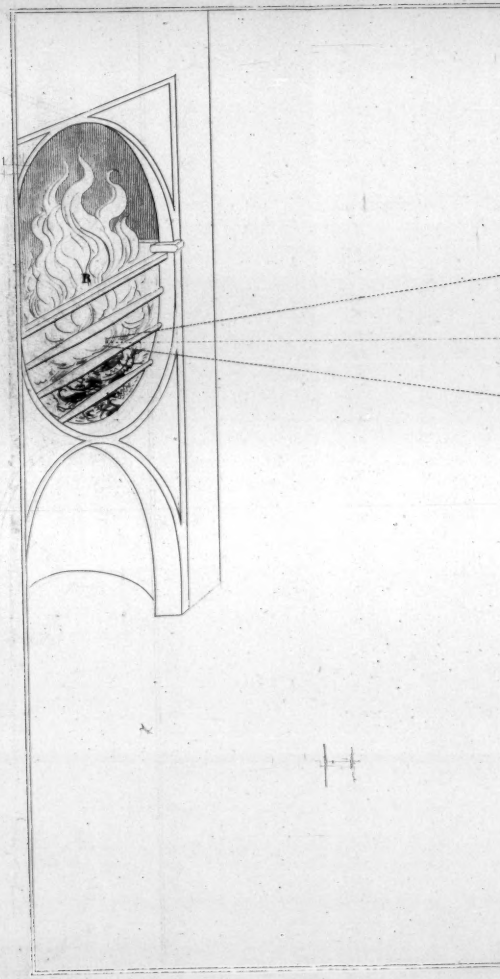
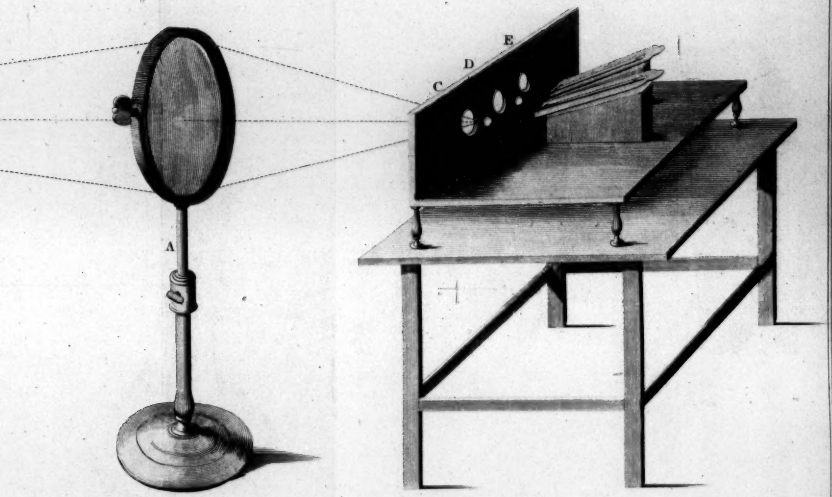



A
C
D
E

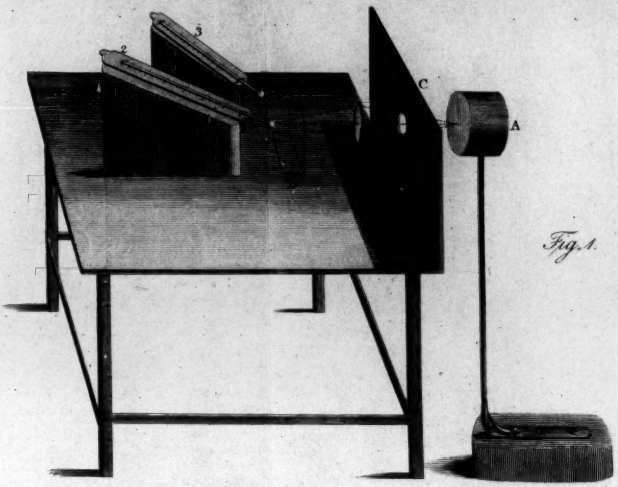


Fig. 1.

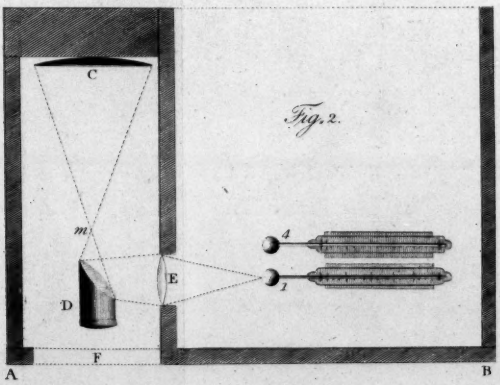


Fig. 2.

XVI. *Chemical Experiments on Zoophytes; with some Observations on the component Parts of Membrane.* *By* Charles Hatchett, *Esq. F. R. S.*

Read June 12, 1800.

The experiments and observations on shell and bone, which I had the honour last year to lay before this learned Society, were made in consequence of my having discovered, some little time before, that the enamel of teeth did not consist principally of carbonate of lime, as was generally believed, but was of a nature similar to bone; with this difference, that the phosphate of lime was not deposited in and upon a cartilaginous or membranaceous substance, but was only blended with a certain portion of animal gluten.* By the experiments subsequently made on various shells, crustaceous substances, and bones, it was proved,

1st. That the porcellaneous shells resemble the enamel of teeth in the mode of formation, but that the hardening substance is carbonate of lime.

2dly. That shells composed of nacre or mother of pearl, or approaching to the nature of that substance, and also pearls,

* See experiments mentioned in Observations on the teeth of graminivorous quadrupeds, &c. by Everard Home, Esq. F. R. S. Phil. Trans. for 1799, p. 243.

resemble bone in a considerable degree, as they consist of a gelatinous, cartilaginous, or membranaceous substance, forming a series of gradations, from a tender and scarcely perceptible jelly to membranes completely organized, in and upon which carbonate of lime is secreted and deposited, after the manner that phosphate of lime is in the bones; and therefore, as the porcellaneous shells resemble the enamel of teeth, so the shells formed of mother of pearl, *&c.* in like manner resemble bone; the distinguishing chemical character of the shells being carbonate of lime, and that of enamel and bones being phosphate of lime.

3dly. It was proved, that the crust which covers certain marine animals, such as crabs, lobsters, crayfish, and prawns, consists of a strong cartilage, hardened by a mixture of carbonate and phosphate of lime; and that thus these crustaceous bodies occupy a middle place between shell and bone, although they incline principally to the nature of shell. And,

4thly. It was proved, that a certain portion of carbonate of lime enters the composition of bones in general; the proportion of it however being, to the phosphate of lime, *vice versâ* to that observed in the crustaceous marine substances.

Upon the view, therefore, of these facts, it is evident, that there is a great similarity in the construction of shell and bone; and that there is even an approximation in the nature of their composition, by the intermediate crustaceous substances.

These remarks, with the experiments by which they are supported, form the principal features of that paper, which the Royal Society honoured with a place in the last volume of the Philosophical Transactions. At that time, it was not my

intention immediately to pursue the subject; but I changed this resolution, after a conversation with my friend Dr. GRAY, Sec. R. S. who suggested, that many marine substances still remained to be examined in a similar manner; and that a series of experiments on Zoophytes (hitherto but little known in respect to their component parts) would be very interesting, and might probably lead to some improvement in their classification.

I was therefore induced to make the experiments contained in the following pages; and, as the mode adopted was very similar to that which was formerly pursued, it appears superfluous here to repeat the description.

It will be proper, however, to observe, that argill is not unfrequently lodged, as an extraneous substance, in the interstices of many of the Madrepores, and such like bodies; and, as argill is precipitated by pure ammoniac, it became necessary not to rely merely on the ammoniac, as a test of phosphate of lime.

Whenever, therefore, any precipitate was produced by ammoniac, it was dissolved again in acetous acid, and this solution was examined by the addition of acetite of lead.

§ I. EXPERIMENTS ON ZOOPHYTES.

*Madrepora virginea.**

This Madrepore, when immersed in very dilute nitric acid, effervesced much, and was soon dissolved.

The solution was perfectly transparent and colourless, with

* The different species are named according to GMELIN's edition of LINNÆUS's *Systema Naturæ*.

but a small appearance of gelatinous or membranaceous particles. Pure ammoniac was then added, but did not cause any alteration; and the whole of what had been dissolved, was afterwards completely precipitated by carbonate of ammoniac, and proved to be carbonate of lime.

Madrepora muricata.

When treated like the former, it afforded some loose particles of a gelatinous substance: these were separated by a filter, and the solution was supersaturated with pure ammoniac, without effect; but, upon adding carbonate of ammoniac, the dissolved part was precipitated, in the state of carbonate of lime.

Madrepora labyrinthica.

This, being examined in the manner abovementioned, proved to be composed of carbonate of lime, and of a loose gelatinous substance, similar to that afforded by *Madrepora muricata.*

Madrepora ramea.

When this Madrepore was first immersed in very dilute nitric acid, a considerable effervescence was produced; and, after a few hours, a pale brown fibrous membrane remained, which in some measure exhibited the original figure of the Madrepore. The clear solution being poured into another vessel, only afforded a large quantity of carbonate of lime.

Madrepora fascicularis.

When this was put into very dilute nitric acid, a considerable effervescence arose; and, after some hours, a tender membrane

was left, which retained the original shape. Pure ammoniac did not disturb the transparency of this solution; but a copious precipitate of carbonate of lime was obtained, by the addition of carbonate of ammoniac.

These experiments, on only a few of the Madrepores, sufficiently prove how similar they are, in composition, to shell; for both consist of the same materials, subject to the like modifications.

Millepora cærulea.

This produced much effervescence, when immersed in very dilute nitric acid. The blue colour disappeared, as the calcareous part was dissolved, and was not afterwards restored by ammoniac.

Some loose detached portions of a gelatinous substance floated in the solution, which were separated by a filter. The transparency of the solution was not disturbed by pure ammoniac; but a copious precipitate of carbonate of lime was produced by carbonate of potash.

Millepora alcicornis.

This Millepore, when treated with very dilute nitric acid, produced a great effervescence; and, after a few hours, a tender gelatinous substance remained, which did not retain the figure of the Millepore.

Pure ammoniac had not any effect; but carbonate of ammoniac precipitated a large quantity of carbonate of lime.

Millepora polymorpha.

This produced an effervescence, when put into dilute nitric

acid; and, after some hours, a substance remained, which completely retained the original figure of the Millepore.

The substance which thus remained, was composed of a strong white opaque membrane, which formed the external part; the interior of this, was filled with a transparent gelatinous substance. Ammoniac produced a very slight precipitate, which, being dissolved in acetous acid, was proved to be phosphate of lime, by solution of acetite of lead. Carbonate of soda afterwards precipitated a large quantity of carbonate of lime.

Millepora cellulosa.

This Millepore effervesced much with dilute nitric acid; and, when this had ceased, a finely perforated membrane remained, in structure and appearance like the original substance.

Ammoniac did not produce any effect; but a large quantity of carbonate of lime was obtained by carbonate of soda.

Millepora fascialis.

This resembled the former in every particular; and left a membrane perfectly like the Millepore.

Millepora truncata.

When treated with dilute nitric acid, it effervesced much, like the former; and, after a few hours, a semi-transparent membranaceous substance remained, which exhibited completely the shape and structure of the original Millepore.

Ammoniac did not disturb the transparency of the solution; but the whole of the dissolved portion was precipitated, in the state of carbonate of lime, by carbonate of ammoniac.

The remark lately made on the Madrepores may here also

be repeated, as the composition of the Millepores appears to be the same, with the single exception of *Millepora polymorpha*, which afforded some slight traces of phosphate of lime. But time and future experiments will shew whether this was an accidental circumstance, or whether the *Millepora polymorpha* is thus distinctly characterised.

It is likewise necessary to add, that when these various Madrepores and Millepores were exposed to red heat, in a crucible, they emitted smoke, with the smell of burned horn or feathers, became tinged with a paler or deeper gray colour, and (when dissolved in acids) deposited more or less animal coal, in proportion to the quantity of the gelatinous or membranaceous substance detected by the experiments lately described.*

Tubipora musica.

Of the *Tubiporæ*, I had only an opportunity to examine this species.

Like the former substances, it was immersed in an acid, and on this occasion I employed the acetous acid. A great effervescence was produced, and the red colour was destroyed, in proportion as the calcareous part was dissolved. When the solution was completely effected, some loose particles of a tender membrane floated in the liquor, and were separated by a filter.

Pure ammoniac, added to the solution, produced a precipitate; which proved to be argill, accidentally lodged in the interstices of the Tubipore.

* The order in which these experiments are placed, is not that according to which they were made; but it has been adopted, because it shews more evidently the gradations of the membranaceous substances.

To the filtrated liquor, carbonate of potash was added, and precipitated a large quantity of carbonate of lime.

Flustra foliacea.

When this was immersed in very dilute nitric acid, an effervescence of short duration took place; and, when this had ceased, the *Flustra* appeared like a finely reticulated membrane, which retained the original shape.

Pure ammoniac being added to the filtrated solution, produced a slight precipitate; which, being dissolved in acetous acid, was proved, by acetite of lead, to be phosphate of lime.

Solution of carbonate of ammoniac was then added to the liquor from which the phosphate of lime had been separated, and produced a copious precipitate of carbonate of lime.

When the *Flustra foliacea* was exposed to a low red heat, in a crucible, it emitted a smell like burned horn, but retained its shape, by reason of the carbonate of lime with which it was coated. The *Flustra* thus burned, when dissolved in dilute nitric acid, deposited some animal coal; but, in other respects, the present solution resembled the former nitric solution of this substance when in a recent state.

The *Flustra foliacea*, when long digested with boiling distilled water, communicated to it a pale brownish tinge. Infusion of oak bark being then poured into the liquor, did not produce any visible effect, even after 24 hours had elapsed; but nitro-muriate of tin formed a white cloud, in the space of a few minutes.

Corallina Opuntia.

This being put into very dilute nitric acid, produced (like the *Flustra foliacea*) an effervescence of short duration.

The coralline then remained in a membranaceous state, and retained the original figure. To the filtrated solution some pure ammoniac was added, but it scarcely produced any visible effect.

Carbonate of ammoniac precipitated a large quantity of carbonate of lime.

Some of the *Corallina Opuntia* was then exposed to a low red heat, in a crucible; it emitted a smell of burned horn, and in great measure retained its shape, evidently from the calcareous coating.

The burned coralline, being dissolved in dilute nitric acid, deposited some animal coal.

The clear solution afforded, by pure ammoniac, a very slight precipitate of phosphate of lime; after which, the carbonate of lime was precipitated as before.

This coralline, when treated with boiling water, like the *Flustra foliacea*, did not discolour it; neither was the water changed by infusion of oak bark; but nitro-muriate of tin produced a faint white cloud.

Isis ochracea.

When this *Isis* was immersed in dilute nitric acid, a considerable effervescence was produced; and, in proportion as the calcareous substance was dissolved, the red colouring matter was deposited, in the state of a fine red powder.*

When the effervescence had ceased, (which was after about three hours,) a yellowish membrane remained, which completely retained the original figure of the *Isis*.

* This colouring substance was not dissolved, nor changed, when nitric or muriatic acids were poured upon it. It appears, therefore, to be very different from the tinging matter of the *Tubipora musica*, or that of the *Gorgonia nobilis*.

The solution, being filtrated, was saturated with pure ammoniac, by which, a slight precipitate of phosphate of lime was separated. A large quantity of carbonate of lime was afterwards precipitated, by solution of carbonate of potash.

Part of a branch of this *Isis* was put into a crucible heated to a low red heat. A great quantity of smoke was emitted, which had the smell of burned horn; and, after a few minutes, the branch separated at the knotty joints, into as many pieces as there were joints in the branch. These joints had all the characters of coral; but the whole of the membrane which had invested them, as well as the knotted protuberances by which they had been connected, were destroyed, by being converted into coal. From this circumstance, I was desirous to examine the internal structure of the membranaceous part, out of which these joints of coral had been dissolved by acids.

I took, therefore, the membranaceous substance which remained after the first experiment, and which (as I have already observed) retained the complete figure of the *Isis*.

This substance being opened longitudinally, exhibited a series of cavities, corresponding in form with the coralline joints, and so situated, that each of these cavities extended from one bulb or knot nearly to the next, throughout the whole of the branch.

The coralline joints, when viewed separately, appeared smaller in the middle than at the ends, which were terminated by obtuse cones.

In the branch, these joints were so placed, that the extremities or cones were opposed point to point; but were prevented from immediate contact, by a gristly substance, which filled, and indeed principally formed in the branch, the knot or bulb of

each joint, and was interposed between the cones of the coralline substance, like the common cartilages of the articulations.

From this construction it appears, that this *Isis* is capable of great flexibility when in a recent state; for the gristly part of the bulbs is then most probably much softer, and more elastic, than it appears to be in the dried specimens which are found in collections.*

The gristly substance which forms the bulbs, and the coralline joints, are kept together, and are covered, by a thin skin or membrane, which is continued over the whole, like a tube. The joints are not, therefore, devoid of a coating, as seems to be implied by the definition of LINNÆUS.

Isis Hippuris.

Part of a branch of this *Isis* was immersed in very dilute nitric acid, and a considerable effervescence immediately took place. When this effervescence had ceased, there appeared little or no change in the original form of the *Isis;* but the coralline joints were now become a soft, compact, white, and opaque membranaceous substance; while the dark brown intermediate parts retained also their form, and in other characters resembled those of horn.

The solution was colourless and transparent. When saturated with pure ammoniac, it was not affected; but carbonate of potash produced a copious precipitate of carbonate of lime.

Another part of this *Isis* was exposed to a low red heat, in a

* It must here be observed, that the articulated structure abovementioned, is not to be found in those parts which form the main stem, with its larger branches; the joints in those parts being consolidated, so as to constitute a strong and rigid trunk, upon which the whole fabric is supported.

crucible. The dark brown horny parts swelled, and puffed up, with much smoke, and a smell like that of burned horn.

The coralline joints also emitted the same smoke and smell, and became dark gray.

When put into dilute nitric acid, a solution was made, with effervescence, during which, there was a copious deposition of animal coal.

From this solution, nothing but carbonate of lime was obtained by the usual precipitants.

A tube of membrane invests the curious structure of the *Isis ochracea;* but no such tube or outer coat exists in the *Isis Hippuris;* for the coralline joints (like some of the Madrepores, Millepores, *&c.*) consist of a membranaceous substance, hardened by carbonate of lime, and the only difference appears to be, that in the Madrepores and Millepores, the membranaceous part is less compact and abundant; but, even the striæ of the coralline joints remain visible, and unchanged, in the membrane of this *Isis.*

The brown horny part forms also a marked characteristic in this *Isis,* and seems to approach it to certain of the *Gorgoniæ.*

This horny part does not however pervade the whole of the branch; for, where the coralline joints commence, this horny substance immediately terminates, internally as well as externally, and is not to be discovered but between or in the separation of these joints.

Gorgonia nobilis.

I next proceeded to examine the *Gorgonia nobilis* or red coral; and, of this, I separately subjected to experiment different pieces,

some of which were polished, and deprived of their external, pale red, mealy coat, whilst others were in their original state.

A piece of the unpolished red coral being put into dilute nitric acid, an effervescence immediately took place; and, after some hours, the whole of the calcareous substance was completely dissolved.

The external coat retained the original figure, and appeared like a pale yellow tubulated membrane, the interior of which was filled with a transparent gelatinous substance. From the solution I only obtained a large quantity of carbonate of lime.

The next experiment was made in a manner exactly similar to the former; but a piece of the polished or uncoated red coral was now taken.

The effects produced by the diluted acid were the same as before; but, in the solution, some loose portions of a transparent yellow gelatinous substance were now only to be seen.

The filtrated solution was treated as in the former experiment, and afforded a considerable quantity of carbonate of lime.

As it was possible that the action of the nitric acid (although much diluted) might be too powerful, I was induced to try the effects of acetous acid, in which I immersed a piece of the red coral in its natural state. It was gradually dissolved, with a slow effervescence, and left an external tubulated membrane, retaining the original form, and filled with a transparent gelatinous substance, as in the first experiment.

The solution, when filtrated, afforded carbonate of lime.

A piece of the polished or uncoated red coral was treated with acetous acid, in a similar manner.

It was slowly dissolved, and left a transparent gelatinous

substance, like that which has already been mentioned, excepting that it was not in detached portions.

This solution, like the former, only yielded carbonate of lime.

It may here be observed, that in each of the above related experiments, the red colour of the coral was gradually destroyed, as the solution of the calcareous substance advanced, and could not afterwards by any means be restored; nor could any colouring principle whatever be detected by the re-agents usually employed.

A piece of red coral, in its natural or uncoated state, was exposed to a low red heat, in a crucible, during about ten minutes, at which time a faint smell of burned horn was to be perceived. When the coral was taken out of the crucible, it had completely lost the red colour, and was become pale gray. It dissolved in dilute nitric acid, with effervescence, and some animal coal was separated.

To the filtrated solution pure ammoniac was added, and produced a very slight precipitate, which was collected, and was afterwards dissolved in acetous acid. From this solution, by the addition of acetite of lead, some phosphate of lead was obtained.

The carbonate of lime was afterwards precipitated in the usual manner.

As the very small portion of phosphate of lime discovered in the preceding experiment, (and which had escaped the action of the acids then employed,) might be only contained in the coating or epidermis, a piece of the polished or uncoated coral was treated in a similar manner; but, upon examining the solution, it afforded a small portion of phosphate, with a large

quantity of carbonate of lime; so that the result of this experiment did not differ from that of the former.

From the preceding experiments it appears, that the *Gorgonia nobilis* or red coral, consists of two parts; one of which is the stem, formed of a gelatinous substance, hardened by carbonate of lime, and coloured by some unknown modification of animal matter; the other is a membranaceous tube, which, like a cuticle or cortex, coats the stem abovementioned, and (when deprived of its hardening substance) possesses all the characters of membrane. But, although carbonate of lime could only be discovered, when this *Gorgonia* was simply immersed in acids, yet it has been proved by these experiments, that a small portion of phosphate of lime is also present, but so enveloped by the membranaceous and gelatinous parts, as not to be dissolved by the acid menstrua, till these substances have been decomposed by fire.

This is not an unusual circumstance, when a very small portion of a substance is enveloped by large quantities of other matter; for, BERGMANN, in his supplement to SCHEELE's Essay on the *Calculus Vesicæ*, observes, that the presence of calcareous earth in certain calculi, could not be discovered in the usual manner, but by operations made expressly for the purpose.*

I do not pretend to determine whether the very small portion of phosphate of lime in the *Gorgonia nobilis* is an essential ingredient or not; but the mode of construction evidently proves how much this *Gorgonia* differs from the Madrepores and Millepores, as well as from the *Gorgoniæ* about to be mentioned.

* SCHEELE's Essays, translated by Dr. BEDDOES. Page 208.

Gorgonia ceratophyta.

When this was immersed in dilute nitric acid, an effervescence was produced; after which, the cortical part appeared like a thin yellowish membrane investing the stem, which was become transparent, and similar to cartilage.

Ammoniac precipitated from the solution a large quantity of phosphate of lime; and lixivium of potash separated some carbonate of lime.

A quantity of the cortex (which had been separated from the stem by beating it between folded writing paper) was steeped in the dilute acid. This solution afterwards, with ammoniac, scarcely afforded a vestige of phosphate of lime; but, when lixivium of carbonate of potash was added, a considerable quantity of carbonate of lime was obtained.

The stem, on the contrary, when thus treated, afforded much phosphate of lime, and very little of the carbonate. When burned in a crucible, it smoked, and emitted a smell like burned horn, but the figure was not destroyed; and, when afterwards dissolved in the acid, it yielded the same products as before.

Gorgonia Flabellum.

When this *Gorgonia* was steeped in dilute nitric acid, it produced an effervescence of short duration. The cortical part then appeared like a thin yellowish membrane, which covered the stem.

The latter was transparent, and resembled softened horn of a reddish brown colour.

The solution afforded a large quantity of phosphate of lime,

by the addition of ammoniac; after which, lixivium of potash formed a less copious precipitate of carbonate of lime.

Some parts of the cortex were separated, by beating the *Gorgonia* between folded writing paper, and were immersed in the acid.

This solution was scarcely rendered turbid by ammoniac; but afforded a considerable portion of carbonate of lime by potash.

The stem from which the above cortical part had been separated, was next examined by the dilute acid, in which, when steeped during three days, it became soft, elastic, and in some measure cartilaginous.

The acid was saturated with pure ammoniac, and then changed to a deep yellow or orange colour: a large quantity of phosphate of lime was at the same time separated; and but very little carbonate of lime was afterwards precipitated by potash.

The recent stem in great measure retained its shape, when put into a red-hot crucible; but that which had been steeped in the acid, curled up, and soon became a shapeless mass of coal, which, by a longer continuation of the red heat, was completely dissipated.

This difference appears to have been caused by the phosphate of lime, which was present in the recent stem, but was dissolved and separated in the latter case by the acid.*

These experiments prove, that the *Gorgonia Flabellum*, like the *Gorgonia ceratophyta*, consists of a horny stem, containing a certain portion of phosphate of lime; and that this stem is invested with a membrane, hardened principally by carbonate of

* These different effects are to be observed, when bone, and when the cartilage or membrane which remains after bone has been long steeped in acids, are subjected to a red heat.

lime, which serves to cover and defend it, in the manner of a shell.*

Gorgonia suberosa.

The cortical part of this *Gorgonia* was separated from the stem, and was first subjected to experiment.

Some portions of this cortex were immersed in dilute nitric acid; and, after an effervescence which continued several hours, a soft yellowish membranaceous substance remained, retaining the original figure.

The liquor, when decanted, was pale yellow, which colour was much deepened by the addition of ammoniac: at the same time, a small quantity of phosphate of lime was deposited.

A considerable portion of carbonate of lime was afterwards precipitated by carbonate of potash.

Some pieces of the cortex were boiled with distilled water during about six hours; and, to the filtrated liquor, infusion of oak bark was added, by which, a large quantity of gelatin was precipitated.

The same pieces were afterwards boiled with lixivium of caustic potash, which effected a perfect solution, and formed the animal soap of CHAPTAL; at the same time, the calcareous matter subsided to the bottom of the matrass.

The cortical part of this *Gorgonia*, when put into a red-hot crucible, emitted much smoke, with a smell like horn that is

* It may here be proper to observe, that the membranaceous part of all these substances, such as the Madrepores, Millepores, *Flustra*, *&c. &c.* was dissolved, when these bodies were boiled with lixivium of caustic potash; and animal soap was formed. The same may also be said of shells; and Mr. VAN MONS has noticed this effect on those of the oyster. See *Annales de Chimie*, Tome XXXI. p. 123.

burned; after this, it fell into pieces, which, being dissolved in nitric acid, afforded a small portion of phosphate of lime, and a large quantity of carbonate of lime.

When the stem of this *Gorgonia* was steeped during 14 or 15 days in dilute nitric acid, it tinged it with pale yellow. The stem, after this, appeared more transparent and flexible, so as to approach the characters of cartilage.

The yellow liquor was changed to a deep yellow or orange colour by the addition of ammoniac; but did not yield any precipitate, even when carbonate of potash was added.

Part of a stem was cut into small pieces, and was boiled for several hours with distilled water.

When filtrated, the water had acquired a very pale yellow tinge; and, upon the addition of infusion of oak bark, yielded a slight precipitate of gelatin.

Lixivium of caustic potash was then poured upon the same pieces; and, being boiled, a thick dark-coloured viscid substance was formed, which possessed all the characters of CHAPTAL'S animal soap.

When the stem of this *Gorgonia* was exposed to a red heat in a retort, or crucible, it curled up, and smelled like burned horn; after which, a spongy coal remained, of difficult incineration.

By a long continuation of the heat, a residuum was left, so small as scarcely to be collected, which, being dissolved in dilute nitric acid, afforded, by the addition of ammoniac, a slight precipitate of phosphate of lime.

Another species of *Gorgonia*, which much resembled the *suberosa*, (excepting that the cortical part was much larger in proportion to the stem,) was next subjected to examination,

and proved to be of a similar composition with those already mentioned.

Gorgonia pectinata.

The cortical part of this *Gorgonia* effervesced with dilute nitric acid, and left a soft yellowish white membrane.

Ammoniac precipitated a small quantity of phosphate of lime; after which, a copious precipitate of carbonate of lime was obtained by potash.

The stem, in its habits, resembled those which have been described.

Gorgonia setosa.

An effervescence was produced upon the immersion of this *Gorgonia* in dilute nitric acid; and, after some hours, the cortical part appeared like a thin yellowish membrane, which coated the horny stem.

The acid solution, upon the addition of ammoniac, yielded a slight precipitate of phosphate of lime; and a large quantity of carbonate of lime was afterwards obtained by potash.

When the cortex was separately steeped in the acid, and the solution examined in the way so often mentioned, only carbonate of lime was obtained.*

On the contrary, the stem (whether recent or burned) afforded a small portion of phosphate of lime, but scarcely any trace of carbonate. The stem which had been long steeped in the acid, became soft and transparent, like a cartilaginous or tendinous substance.

* When the cortical part had been long digested in boiling distilled water, a brownish solution was formed, which was but little affected by infusion of oak bark; but nitro-muriate of tin produced a precipitate.

The *Gorgoniæ* which have been enumerated, much resemble each other in the composition of their cortices, as well as in the nature of their stems.

In the cortex, the predominant hardening substance is carbonate of lime; but, in the stem, phosphate of lime is the chief and almost the only earthy substance that is present.

The following *Gorgoniæ* (although in like manner invested by a cortex) are different, as they do not afford any phosphate of lime.

Gorgonia Umbraculum,
Gorgonia verrucosa,

and three other species not described, so much resemble each other in their chemical characters, that it would be superfluous to give a separate account of them.

The cortical parts of these *Gorgoniæ* were separately immersed in dilute nitric acid. An effervescence immediately took place, and, after some time, they were found in the state of soft, pulpy, yellowish white membranaceous bodies, retaining nearly their original size and form.

The acid solutions did not afford any phosphate of lime when ammoniac was added; but a large portion of carbonate of lime was precipitated by solution of potash.

The stems of these *Gorgoniæ*, when immersed during 14 days or more in the dilute acid, were very little affected, excepting, that they became softer and transparent, so as to approach the characters of cartilage or softened horn.*

* The stems of the various *Gorgoniæ*, which had been thus softened by long immersion in dilute nitric acid, became of a deep reddish orange colour, inclining to

The acid in which they had separately been steeped, did not afford any precipitate by the addition of the alkalies; and the only change was in the colour, which became deep yellow when ammoniac was added.

The *Gorgoniæ* now to be mentioned, differ from the former, as they are not coated with a fleshy or pulpy cortical substance. They are here placed immediately before the *Antipathes*, on account of their great similarity in chemical properties, as well as in external appearance.

Gorgonia Antipathes.

Some pieces of this *Gorgonia* were immersed in dilute nitric acid during three weeks, at the end of which time they were much softened, and appeared to be composed of a pale brown, opaque, membranaceous substance, which formed concentrical coats, of a ligneous aspect.

The acid in which these pieces had been steeped, was become pale yellow, and changed to orange colour when ammoniac was added; but not the smallest precipitate could be thus obtained; nor was any alteration caused by the addition of lixivium of potash.

When distilled water was boiled with the *Gorgonia Antipathes* during about six hours, it became slightly tinged with yellow; and, some infusion of oak bark being added, a small quantity of gelatin was precipitated.

The pieces of this substance which had been thus treated, were afterwards boiled with lixivium of caustic potash, by which

brown, when subsequently steeped in pure ammoniac; and, in the course of a few hours, they were completely dissolved.

the whole was dissolved, and a very dark coloured animal soap was formed.

When this *Gorgonia* was exposed to a red heat, it emitted much smoke, with a smell of burned horn: it soon lost its shape, puffed up, and formed a spongy coal, which, by a long continued heat, left a few particles of a white substance, consisting chiefly of muriate of soda.

Another species of *Gorgonia* was next examined, the stem of which is from one quarter to nearly half of an inch in diameter in the thickest parts; of a black colour, and a high polish, like black sealing wax: it has probably been considered as a variety of *Gorgonia Antipathes*.

This, by immersion during 28 days in dilute nitric acid, gradually became semi-transparent, and of a bright brownish yellow. In this softened state, it was steeped two days in water, and was then opened longitudinally. By this, the whole structure became apparent, and consisted of thin coats or tubes of a beautiful transparent membrane, which, beginning from a central point, progressively became larger, according to the order by which they receded from the centre.

These membranes were so delicate, that the fibrous texture could scarcely be discerned.

The acid in which this species had been steeped, was tinged with very pale yellow. Ammoniac being added, changed it to a deep yellow or orange colour; but the transparency of the liquor was not disturbed by this, or any of the other precipitants which had been employed in the former experiments.

When this *Gorgonia* was exposed to a red heat, it crackled, and emitted a thick smoke, with the smell of burned horn. The shape was soon destroyed, and a compact coal remained.

By continuing the red heat, a very small portion of white matter was obtained, which, as far as the quantity would allow, was proved to be muriate of soda, with some carbonate of the same.

The last species of *Gorgonia* which I shall here mention, is one which so much resembles the *Gorgonia Antipathes* as not easily to be distinguished from it, and, like the preceding, has probably been confounded with it; but, upon closely comparing them, the *Gorgonia* now treated of is found to be more flat in the stem, on the thin sides or edges of which, a number of short spines or protuberances are placed very near each other. That it is very different from the *Gorgonia Antipathes*, will be proved by the subsequent experiments.

Some pieces of this *Gorgonia* were exposed to the action of dilute nitric acid for nearly four weeks.

The structure then became very apparent, and consisted of strong fibres, which were placed nearly in a parallel direction, from one extremity of the branch to the other, and, being closely arranged side by side, formed concentric coats of a pale brown opaque substance; but these coats were by no means so distinct as those observed in the *Gorgoniæ* formerly mentioned, although, like them, the fibrous substance possessed the characters of membrane.

The dilute acid in which these pieces had been steeped, was become pale yellow, which changed to orange colour when ammoniac was added; at the same time, so large a quantity of phosphate of lime was precipitated, that the liquor became thick and viscid.

The phosphate was separated by a filter; and lixivium of potash was added to the clear liquor, without producing any effect.

This *Gorgonia* was digested in boiling distilled water during 18 hours, and tinged it with pale yellow.

Infusion of oak bark was then poured into the liquor, and precipitated a small portion of gelatin.

The pieces employed in the above experiment were next boiled with lixivium of caustic potash, and formed a dark-coloured animal soap; at the same time, the phosphate of lime was separated, and was gradually deposited at the bottom of the matrass.

Part of a large branch of this *Gorgonia* was exposed to a low red heat.

It immediately emitted a thick smoke, with the smell of burned horn; and, after a long continued heat, the phosphate of lime was left, so as to retain the original figure, like bone which has been burned; but, in the present instance, the particles of the mass cohered but feebly.

When this residuum was dissolved, and the phosphate separated in the usual manner, a slight cloud of carbonate of lime was produced by potash.

Antipathes Ulex.

When this had been immersed 14 days in dilute nitric acid, it became transparent, and so much softened, that from a horny substance it now nearly resembled cartilage. The acid in which it had been steeped, changed to a very deep yellow or orange colour when ammoniac was added; but no precipitate could be obtained by this, or by potash.

A portion of this *Antipathes* was digested with boiling distilled water, from which some gelatin was precipitated afterwards by infusion of oak bark.

The *Antipathes* being then boiled with lixivium of caustic potash, was completely dissolved, and formed the animal soap of Mr. CHAPTAL.

Antipathes myriophylla.

This was subjected to experiments like those above related, and, as the effects were the same, it is not necessary that particular mention should here be made of them.

As the specimens of these *Antipathes* were small, I was not able to make any additional experiments; but what has been said sufficiently proves how much they resemble the horny stems of the *Gorgoniæ*.

Sponges.

Many species of Sponge were examined, but, as little or no essential difference was found in the results, I shall include all of them in what is now to be related.

The following species, with many others not described, were subjected to experiment.

Spongia cancellata.
Spongia oculata.
Spongia infundibuliformis.
Spongia palmata.
Spongia officinalis.

When the Sponges had been immersed in nitric acid (diluted with three measures of distilled water) during 14 or 16 days, the acid became pale yellow, and was changed to an orange colour by the addition of pure ammoniac.

The Sponges which had been thus steeped in the dilute acid,

became (like the *Gorgoniæ*) more or less transparent, and were considerably softened. In this state, if they were touched with ammoniac, the part thus touched became of a deep orange colour, inclining to a brownish red; and, when much softened by the acid, (if afterwards immersed in ammoniac,) they were completely dissolved, and formed a deep orange-coloured solution.*

When digested with boiling distilled water, the Sponges afforded a portion of animal jelly or gelatin, which was precipitated by infusion of oak bark.

The fine and more flexible Sponges yielded gelatin in greater abundance, and more easily, than those which were coarse and rigid. The gelatin was gradually and progressively imparted to the water, and seems (even in the same Sponge) to be a constituent principle, of different degrees of solubility; and it must be noticed, that in proportion as the Sponges (particularly those which were soft and flexible) were deprived of this substance, in the like proportion they became less flexible and more rigid, so that the remaining part, when dry, crumbled between the fingers; or, when moist, was torn easily, like wetted paper.

As the above properties prove that Sponges only differ from the horny stems of the *Gorgoniæ*, and from the *Antipathes*, by being of a finer and more closely woven texture,† so this similarity will be corroborated by the following remarks.

When exposed to heat, they yielded the same products, the

* The same effects were observed when the horny stems of the *Gorgoniæ*, *Antipathes*, &c. (which had been long steeped in dilute nitric acid,) were immersed in pure or caustic ammoniac.

† This is particularly to be observed by comparing the coarse sponges (such as *Spongia cancellata*) with the finely reticulated parts of certain *Gorgoniæ*, especially those of *Gorgonia Flabellum*, when divested of the external membrane.

same smell, and afforded a similar coal, which by incineration left a very small residuum, consisting chiefly of muriate of soda, occasionally mixed with some carbonate of lime, which was also often discovered when the recent Sponges were immersed in acids; but this, as well as the muriate of soda, is I believe merely extraneous, and arises from small shells, parts of Madrepores, and such like bodies, which are often visibly lodged in the interstices of the Sponges.

Lastly, the Sponges, when boiled with lixivium of caustic potash, were completely dissolved, and, like the horny stems of the *Gorgoniæ*, formed animal soap, more especially when the part which is apparently insoluble in water, and which remains after the gelatin has been separated, was thus treated.

Alcyonium asbestinum.

This, after being immersed during several hours in dilute nitric acid, remained unchanged in figure; a feeble effervescence was at first produced, and the reddish purple colour was destroyed.

The external part became pale yellow, and was a soft opaque pulpy substance, within which was a stem, very similar in texture, but less soft, and which still appeared of a pale red colour.

When pure ammoniac was added to the filtrated solution, no apparent effect was produced; but carbonate of potash precipitated a large quantity of carbonate of lime.

When a piece of this *Alcyonium* was exposed to a low red heat, it soon took fire, and emitted a smell like burned horn; after which, it retained its figure, and became white. Being dissolved in dilute nitric acid, some animal coal was deposited;

and, upon the addition of ammoniac, a small portion of phosphate of lime was obtained, which being separated, the carbonate was precipitated as before.

Some pieces of this *Alcyonium* were digested with boiling distilled water during six hours; the liquor was then decanted, and infusion of oak bark being added, a quantity of gelatin was precipitated.

On the pieces of the *Alcyonium* from which the water had been decanted, some lixivium of caustic potash was poured; and, being boiled, the whole of the membranaceous or pulpy part was dissolved, and a substance exactly similar to CHAPTAL's animal soap was formed, while the calcareous part subsided to the bottom of the vessel.

Alcyonium Ficus.

When the effervescence produced by pouring dilute nitric acid on this *Alcyonium* had ceased, it was found unchanged in shape, and like a strong thick membranaceous substance of a fibrous texture.

Pure ammoniac, added to the acid liquor, precipitated a small quantity of phosphate of lime; after which, a copious precipitate of carbonate of lime was obtained by potash.

Alcyonium arboreum.

This *Alcyonium*, being steeped in the dilute nitric acid, effervesced, and was acted upon like *Alcyonium asbestinum*.

The calcareous part was soon dissolved; but the form of the *Alcyonium* remained unchanged, and still appeared like a pale yellow porous substance, enveloped by a skin or epidermis.

Ammoniac did not disturb the transparency of the solution; but carbonate of lime was obtained by solution of potash.

When exposed to a low red heat, it resembled *Alcyonium asbestinum;* and a solution being subsequently made, afforded some phosphate of lime, with a large portion of carbonate.

As this phosphate had not been discovered in the first experiment, and therefore appeared to have been defended from the action of the acid by the membranaceous part, that experiment was repeated, with this difference, that the acid was made to boil.

A complete solution of the whole was thus made, which, like that of the burned *Alcyonium*, yielded phosphate of lime; and at the same time the liquor became of an orange colour, as soon as the ammoniac was added.

Some pieces of this *Alcyonium* were digested with boiling distilled water, and tinged it with a pale yellow colour. Infusion of oak bark being then added, a large quantity of gelatin was precipitated.

The same pieces were boiled with lixivium of caustic potash, and, when dissolved, formed animal soap.

The calcareous part was separated during the boiling, and subsided in the form of a fine powder.

From the examination of the few species of *Alcyonium* which have been mentioned, it appears, that as the Sponges resemble the horny stems of *Gorgoniæ*, so these, in external and chemical characters, resemble the fleshy or cortical substance which invests some of those bodies; and that they chiefly differ from the *Gorgoniæ*, by being destitute of the horny stem, which in the latter seems to supply the place of bone.

§ II. OBSERVATIONS ON THE FOREGOING EXPERIMENTS.

The simplicity and uniformity of the experiments here described, will not, I flatter myself, render the facts less worthy of

attention; and I must again repeat, that the minutiæ of analysis did not form part of my present plan, which was only to sketch an outline, comprehending the most prominent chemical characteristics of certain bodies appertaining to the animal kingdom, which hitherto had been but little or not at all examined; so that this outline (although defective) might serve as a chain of connection, and as a basis, upon which a more perfect superstructure may in future be gradually raised; and it appeared evident, that this would be most easily and speedily executed, by following a systematical and comparative plan. For this reason, a great part of my attention was directed towards ascertaining, in these animal substances, the presence and general proportions of carbonate and phosphate of lime; these being the materials essentially employed by nature to communicate rigidity and hardness to certain parts of animals, such as shell and bone; and, although some other substances, as magnesia, silex, iron, with some alkaline and neutral salts, might be occasionally present in small proportions, (and indeed were at times detected,) yet, as these appear to have but little influence on the general characters of the bodies which were examined, I did not, for the present, think proper to take particular notice of them.

The next object was, to examine the nature of the substance in and upon which the hardening or ossifying principles were secreted and deposited; and it seemed to me that the best mode to do this, was to compare and examine this substance in the various states in which it appeared, when deprived of the hardening or ossifying matter.

From what was said in the paper on shell and bone, concerning the substance which remained after the carbonate of lime in shells, and after the phosphate of lime in bones, had been

dissolved and separated by weak acids, it is evident that the substance which thus remains, is as various in relative quantity, as it is in those qualities which apparently are produced by the degrees of natural inspissation, and by the progressive effects of organization.

In the *porcellaneous shells*, such as *Cypræa*, &c. this substance was proved to be much less in quantity than in those which were afterwards mentioned; and, although of a quality which (like a cement or gluten) served to bind and connect the particles of carbonate of lime firmly together, so small was the degree of natural inspissation, and so little advanced was the degree of organization, that when the carbonate of lime was dissolved, even by very feeble acids, little or no vestige of jelly, membrane, or cartilage, could be perceived; nor indeed could any be detected, but by the small portion of animal coal which was formed, when these shells had been exposed for a short time to a low red heat.

But, proceeding from shells of this description to others tending to the nature of nacre or mother of pearl, (such as some of the *Patellæ*,) a substance was left *untouched by the acids*, which had the appearance of a yellowish transparent jelly.* So that the substance which served merely as a gluten in the porcellaneous shells, was not only more abundant in these *Patellæ*, but, being more inspissated, was become immediately visible and palpable.

In the common oyster, these qualities were more strongly marked; and, in the river muscle, and in the shells composed

* The term jelly is here employed only to denote the degree of consistency of this substance, which in its nature is very different from the varieties of animal jelly called gelatin.

of the true nacre or mother of pearl, this substance was found not only to constitute a large part of the shell, but even to be more dense, so as no longer to appear gelatinous; and, in addition to these, strong and visible marks of organization were stamped on every part, and a perfect membranaceous body remained, composed of fibres arranged parallel to each other, according to the configuration of the shells.

From these facts, proved by the examination of only a very few (comparatively speaking) of the known shells, it appears that the hardening principle, or carbonate of lime, together with a substance varying from a very attenuated gluten to a tough jelly, and from this to a perfectly organized membrane, concur to form the matter of shell; and, from the result of the experiments, and from all circumstances, there is every reason to believe, that the substance with which, or upon which, the carbonate of lime is mixed or deposited, is of a similar nature, and differs only in relative quantity and density, arising from progressive changes (peculiar to the various species of shells) produced by certain degrees of natural inspissation, and by an organization more or less perfect.

The experiments made on teeth, and on the bones of various animals, elucidated and confirmed the observations made on the nature of shell; for,

1st. The enamel of teeth (in relation to the other bony substances) was proved to be as the porcellaneous shells are to those formed of mother of pearl; the cementing substance of the enamel being a gluten, in the same state, and apparently of a similar nature, with that of the porcellaneous shells. And,

2dly. In certain bones, particularly those of fish, (such as some of the bones of the skate,) the substance which remained

after the solution of the phosphate of lime, was of a gelatinous consistency, and exhibited but very imperfect traces of organization; by the others, however, a completely formed membrane or cartilage was left, retaining the figure of the original bone.

When therefore the component parts of shell and bone are considered, it appears that the essential characteristics are, carbonate of lime for the one, and phosphate of lime for the other; and that their bases consist of the modifications of a glutinous, gelatinous, or membranaceous substance.

I experienced much gratification in tracing the progressive and connected changes in the composition of the various shells and bones; and a considerable increase of pleasure arose, in proportion as the observations made on those bodies were corroborated, and the chain of connection extended, by the developement of the facts resulting from the experiments on Zoophytes, which form the principal subject of this paper.

It will now be proper to review these experiments, and to examine how far they agree with those made on shell and bone, and how far they tend to prove, that these substances are all of a nature closely connected.

The experiments on the Madrepores afforded the following results.

Madrepora virginea, when examined by acids, left but very little of any gelatinous substance or membrane.

Madrepora muricata, and *Madrepora labyrinthica*, afforded loose portions of a transparent gelatinous substance.

Madrepora ramea, and *Madrepora fascicularis*, when deprived of the carbonate of lime by acids, remained in the state of completely organized membranaceous bodies, which exhibited the original figure of the respective Madrepores; and the proportion

of coal afforded by these last, was more abundant than what was obtained from those which were first mentioned.

To these succeeded the experiments on the Millepores; from which it appeared, that

Millepora cærulea afforded loose detached portions of a gelatinous substance.

Millepora alcicornis yielded the same, but in a more coherent state.

Millepora polymorpha remained unchanged in shape, and consisted of a strong, white, opaque membrane, filled with a transparent jelly.

Lastly, *Millepora cellulosa*, *Millepora fascialis*, and *Millepora truncata*, afforded membranaceous bodies, in a complete state of organization; and all these Millepores, when exposed to a low red heat, yielded various quantities of coal, according to the greater or less abundance of the gelatinous or membranaceous substance.

The universal and only hardening principle of these Madrepores and Millepores, was proved to be carbonate of lime, with the single exception of *Millepora polymorpha*, which also appears to be differently constructed from the other *Milleporæ*. With this single exception, carbonate of lime seems to be the only hardening substance in these bodies; and, when every circumstance is considered, an exact similarity is to be found between the substance forming the various shells, and that which forms the *Madreporæ* and *Milleporæ*; and the nature of these bodies is so completely the same, that the changes or gradations of the one are to be found in the other. For the chemical characters which distinguish the porcellaneous shells, are in a great measure approached by those of *Madrepora virginea*; and those

which were noticed in the *Patellæ*, correspond precisely with the Madrepores and Millepores which afford a gelatinous substance; and lastly, the characters of the membranaceous part, exhibited by the shells formed of nacre or mother of pearl, are in like manner to be found among some of the Madrepores and Millepores, such as *Madrepora ramea*, *Millepora fascialis*, *Millepora truncata*; for these, like the *Turbo olearius* and *Haliotis Iris*, are composed of a fibrous membrane, hardened by carbonate of lime.

It appears therefore, that the Madrepores and Millepores, like the various shells, are formed of a gelatinous or membranaceous substance, hardened by carbonate of lime; and the only difference is in the mode according to which these materials have been employed.

The experiments on *Tubipora musica* proved, that in composition it resembled the foregoing substances. But a slight difference was observed, in respect to the hardening substance of *Flustra foliacea* and *Corallina Opuntia*; for a small portion of phosphate was found mixed with the carbonate of lime; but the membranaceous part of these bodies resembled that of certain Madrepores and Millepores, particularly *Millepora fascialis*.

Two species of *Isis* were next examined, namely, *Isis ochracea* and *Isis Hippuris*: both of these were proved to be formed of regularly organised membranaceous, cartilaginous, and horny substances, hardened, in the last mentioned species, merely by carbonate of lime; but, in the *Isis ochracea*, with the addition of a very small portion of phosphate of lime.

The subsequent experiments were made on various species of *Gorgonia*, and first on *Gorgonia nobilis*, which was formerly regarded as an *Isis*.

The hardening substance of this was found to be carbonate of lime, with a small portion of phosphate; but the matter forming the membranaceous part was (like that of *Millepora polymorpha*) in two states; that of the interior being gelatinous; and that of the external part being a membrane completely formed, so as to cover the stem, in the manner of a tube.

The results of the experiments on certain *Gorgoniæ*, such as *ceratophyta*, *Flabellum*, *suberosa*, *pectinata*, and *setosa*, were not a little remarkable; for, when the two parts which compose these *Gorgoniæ* (namely, the horny stem, and the cortical substance by which it is coated,) were separately examined, it was proved,

1st. That the stems of these *Gorgoniæ* consist of a substance analogous to horn; and that, by long maceration in diluted nitric acid, this horny substance becomes soft and transparent, so as to resemble a cartilaginous or tendinous body; moreover, the stems of these *Gorgoniæ* afford a quantity of phosphate of lime, but scarcely any trace of carbonate.

2dly. That the cortical part, on the contrary, consists principally of carbonate of lime, with very little or none of the phosphate; and the carbonate of lime is deposited in and upon a soft, flexible, membranaceous substance, which seems much to approach the nature of cuticle.

Some other *Gorgoniæ*, which were subsequently examined, and which much resembled the former in construction, did not yield any phosphate of lime; but in every other particular they proved to be similar.

The *Gorgonia Antipathes* was found to be entirely formed of a fibrous membrane; and the black shining polished *Gorgonia* afforded, by maceration, a most beautiful specimen of membranes concentrically arranged.

Lastly, the *Gorgonia* which I have described as very much resembling the *Gorgonia Antipathes*, proved to be similar to that species, as to the membranaceous part; but so large a portion of phosphate of lime was mixed with it, as almost to approach it to the nature of stag's or buck's horn; there is, therefore, great reason to consider it as a different species.

The *Antipathes*, which were next examined, were found to be little if at all different from the horny stems of the *Gorgoniæ*. And the various Sponges, which were afterwards subjected to experiment, were proved to be completely formed by the same membranaceous or horny substance, which became varied by the modifications of a more delicate construction, rather than by any essential difference in composition.

This series of experiments terminated with an examination of a few species of *Alcyonium*, namely, *asbestinum*, *Ficus*, and *arboreum*; all of which were found to be composed of a soft, flexible, membranaceous substance, very similar to the cortical part of some of the *Gorgoniæ*, (such as *Gorgonia suberosa*,) and in like manner slightly hardened by carbonate, mixed with a small portion of phosphate of lime.

From what has been said, there is reason to conclude, that the varieties of bone, shell, coral, and the numerous tribe of Zoophytes with which the last are connected, only differ in composition by the nature and quantity of the hardening or ossifying principle, and by the state of the substance with which it is mixed, or connected. For the gluten or jelly which cements the particles of carbonate or phosphate of lime, and the membrane, cartilage, or horny substance, which serves as a basis, in and upon which the ossifying matter is secreted and deposited, seem to be only modifications of the same substance,

which progressively graduates, from a viscid liquid or gluten, into that gelatinous substance which has so often been noticed, and which again, by increased inspissation, and by the various and more or less perfect degrees of organic arrangement, forms the varieties of membrane, cartilage, and horn.

I shall now attempt to prove what I have here asserted, or at least assign the reasons which induce me to adopt this opinion; but, in so doing, I am compelled, from the close connection of the subject, to anticipate the general result of part of a series of experiments, made with a view to investigate the nature and composition of membrane.

To enter into a minute detail of these experiments, would far exceed the limits of a paper like the present; I shall therefore only mention, in a concise manner, the results of those which the subject immediately requires to be brought forward.*

The method which first presents itself in such an investigation, is, the comparative analysis of the different substances, so that their relative proportions of carbon, hydrogen, and azote, should be precisely determined; but, when it is recollected, how long a time would be requisite for making such an immense series of analyses, and how much animal substances are subject to be modified by situation in the body, by age, and by the degree of health of animals, and also that the nature of these, and even that of the unorganised bodies, does not always merely depend on the proportion of the constituent principles, but likewise on the degree and mode of combination to which these principles are subjected; I say that when all this,

* These are the experiments to which I alluded in my former paper, and which I began at the request of my friend Mr. Home, soon after the experiments on the enamel of teeth, &c.

and the plan of the present paper, is considered, I flatter myself that I shall not be censured as hasty or negligent, if at this time I prefer a comparison of the chemical properties of the bodies in question, with those of other substances, which (although not elementary) may be regarded as primary animal compounds; and, when the subject is viewed in its full extent, the mode which I have adopted will, perhaps, be deemed that which is the most satisfactory.

§. III. OBSERVATIONS ON THE COMPONENT PARTS OF MEMBRANE.

In relating the preceding experiments, I have had frequent occasion to remark, that a quantity of that animal jelly which is more or less soluble in water, and which is distinguished by the name of *gelatin*, was obtained from many of the marine bodies, such as the Sponges, the *Gorgoniæ*, and others; but, in the experiments made expressly to investigate the composition of membrane, it still more frequently occurred; and although in many cases, either from the small quantity of the body under examination, or from the very small portion of gelatin thus obtained, I was obliged to content myself with ascertaining the presence of it, by the test of the tanning principle, and by nitro-muriate of tin;* yet, in other experiments, when the solutions of gelatin were gradually reduced by evaporation, I

* Nitro-muriate of tin has been proposed as a test for the tanning principle; and the experiments contained in this paper prove, that it may also be employed with much utility, to ascertain the presence of gelatin, and of certain modifications of albumen.

had opportunities of frequently observing the various degrees of viscidity and tenacity which characterize mucilage, size,* and glue.

The difference in the viscidity and tenacity of the varieties of these substances, is evidently an inherent quality, and not caused by the degree of mere inspissation: if this was the case, mucilage, size, and glue, when dry, would be of an equal quality, which is, however, contrary to daily experience; for the varieties of glue are not of equal tenacity. And it is well known, that glue made from certain parts of animals, such as the skin, is more tenacious, and of a better quality, than that which is made in some places from feet and sinews.

Moreover, when even the same part is employed, which has been taken from two animals of the same species, an evident difference is found, according to the comparative age of the animals; for the best and strongest glue is always obtained from the more aged animals, in whom the fibre is found to be the most coarse and strong. But a longer continued boiling appears requisite in order to extract it; and the more viscid glues are obtained, from the substances which afford them, with greater difficulty than those of a less viscid quality, which may more properly be called size: this difference is to be observed, when muscle is boiled with repeated and frequent changes of water.

Gelatin thus obtained, whether in the state of mucilage, size, or glue, when completely dried, is affected by water according to its degree of viscidity: for, when cold water is poured

* The term size is employed, throughout this paper, to denote that modification of gelatin which appears to be intermediate, between mucilage and the most viscid and tenacious gelatinous substances or glues. The weaker kinds of glue may therefore come under this denomination.

on dry mucilage, it dissolves it in a short time; but, if cold water is poured on those varieties of gelatin which, when dissolved in a proper quantity of boiling water, would, by cooling, form jellies more or less stiff, it acts on them in different degrees, not so much by forming a complete solution, as by causing them to swell and become soft; so that, when a cake of glue has been steeped three or four days in cold water, if it swells much without being dissolved, and, when taken out, recovers its original figure and hardness by drying, such glue is considered to be of the best quality.

I shall soon have occasion to notice, in another place, the effects of acids and of alkalies on gelatin; it will therefore here be sufficient to observe, that as it is soluble in acids, so, if dry mucilage, dry size,* and dry glue, are steeped in nitric acid diluted with three or four parts of water, they will be progressively dissolved, according to the degree of viscidity by which they are separately distinguished.

When the solutions of these substances in water were examined by the tanning principle, and by nitro-muriate of tin, I have found that animal mucilage is more immediately affected by the latter than by the former; while the solutions of size and of glue are equally acted upon by both. And, when gold dissolved in nitro-muriatic acid was added to the solutions of mucilage, size, and glue, the gold was reduced to the metallic state in a few hours, not only on the surface, where it formed a shining metallic pellicle, but also on the sides of the glass, which were thinly coated with a deep yellow sediment, which, like leaf gold, appeared of a fine pale green, when held between the eye and the light.

* Gelatin obtained from eel-skin, evaporated to dryness.

The animal mucilage which I chiefly employed in these experiments, was obtained from the *Corallina officinalis*, as I found it to be pure, and not partly modified into gelatin or animal jelly.* But Mr. BOUVIER asserts, that he obtained the latter substance;† and this appears to me very probable; for mucilage may predominate in this coralline at one period, and gelatin or jelly at another, just as is found to be the case with other animal substances; for it is known, that in young animals mucilage is abundant, and becomes diminished as these increase in growth and age. Hence there is every reason to conclude, that the substance which in the very young animals was at first mucilaginous, becomes progressively more viscid, and assumes the characters of gelatin; which, as animals increase in age, is known to become more and more viscid, as has been already mentioned in the foregoing pages. I am inclined, therefore, to consider mucilage as the most attenuated, and as the lowest in order, among the modifications of gelatin.

As the qualities of gelatin are so various, so the properties of the substances in which it is present as a component part, are much influenced by it; and when, for example, the skins of different animals were compared, I have always found that the most flexible skins afforded gelatin more easily, and of a less viscid quality, than those which were less flexible, and of a more horny consistency.

* By this I mean, that the mucilage had not acquired the degree of viscidity requisite to form a gelatinous substance. The expression which I have employed, is not therefore to be understood as alluding to any essential difference in composition, but only to denote some variation in the degree of consistency; for the whole may be comprehended under the term gelatin, of which, mucilage may be regarded as one extreme, and the strongest and most viscid glue as the other.

† *Annales de Chimie*. Tom. VIII. p. 311.

The skin of the eel possesses great flexibility; and it affords gelatin very readily, and in a large proportion. The skin of the shark also, which is commonly used by cabinet makers to polish their work, was in like manner, for the greater part, soon dissolved, and formed a jelly, like the former. The epidermis or cuticle of these skins, (which is very thin and tender,) although not soluble, was reduced into small particles by violent ebullition; and the spiculæ on the shark's skin were also separated.

The skins of the hare, rabbit, calf, ox, and rhinoceros, were examined in a similar manner, and with the like results; but the gelatin obtained from the hide of the rhinoceros, (as far as the smallness of the piece of skin would allow me to determine,) appeared to be the strongest and most viscid. In every one of these experiments, the true skin or cutis was principally affected, it being completely soluble (as Messrs. CHAPTAL and SEGUIN have well observed) by long boiling; but that of the rhinoceros far exceeded the others in difficult solubility. The cutis of these skins, when first boiled, swelled and appeared horny; it was then gradually dissolved; but in the cutis of the rhinoceros a few small filaments remained, which at length contracted and adhered to the cuticle.

The cuticle of the different skins was softened, but not dissolved; and, as the cutis seems to be essentially formed of gelatin,* so the cuticle appears to contain it, although but in a small proportion: it is, however, necessary to its flexibility; for when, after long boiling, the cuticle of these skins was dried,

* The cartilages of the articulations are also completely soluble when long boiled with water; but this by no means happens when other cartilages are thus treated.

it became a brittle substance, which was easily reduced to a powder.

Hair was much less affected than either of the abovementioned substances; and this, with others in some measure similar, I shall now more particularly notice.

The substances to which I allude, are hair, feather, horn, horny scale, hoof, nail, and the horn-like crust which covers some insects and other animals, such as the scorpion and the tortoise. These I shall now mention, in as concise a manner as the subject will allow.

When hair of various qualities, and taken from different animals, was long digested or boiled with distilled water, it imparted to the water a small portion of gelatin, which was precipitated by the tanning principle, and by nitro-muriate of tin; and, when the hair had been thus deprived of gelatin, and was subsequently dried in the air, the original flexibility and elasticity of it was found to be much diminished, so that it easily gave way, and was broken.

This effect, Mr. ACHARD has also noticed;* and I am induced to believe (from various experiments which I have made on these substances) that the hair which loses its curl in moist weather, and which is the softest and most flexible, is that which most readily yields gelatin; and, on the contrary, the hair which is very strong and elastic, is that which affords it with the greatest difficulty, and in the smallest proportion.

These remarks have moreover been corroborated, by the

* "La perte de la partie gélatineuse ôtant aux cheveux leur souplesse, il s'ensuit "que c'est aux parties gélatineuses qui entrent dans la composition des cheveux qu'ils "doivent leur pliant et leur élasticité."—*Examen chimique des Cheveux, &c. Memoires de l'Acad. de Berlin.* Tom. XXXVIII. p. 12.

assertion of a considerable hair merchant in this metropolis,* who, during a long experience of upwards of forty years, has always found, that hair of the first named quality cannot be boiled an equal time with those last mentioned, without suffering material injury in strength and flexibility.

Feather, digested in boiling distilled water, during ten or twelve days, did not afford any trace of gelatin by the test of the tanning principle; but nitro-muriate of tin produced a faint white cloud.

The same was observed when quill was thus examined.

Shavings and pieces of the horns of different animals were next subjected to experiment, and all afforded small quantities of gelatin, which was precipitated by the tanning principle, and by nitro-muriate of tin; and it was generally observed, that the more flexible horns yielded the largest quantity of gelatin, with the greatest ease; and, (like the substances already mentioned,) when deprived of it, and suffered to dry spontaneously in the air, they became more rigid, and were easily broken.

The horns which I mean, are those of the ox, ram, goat, and chamois, which, in my former paper I considered, as I do now, to be perfectly distinct from the nature of stag's or buck's horn; for this last is as different from the former in chemical composition, as it is in construction: like bone, it affords much phosphate of lime, and, like bone, it affords a large quantity of gelatin; and it is not a little remarkable, that phosphate of lime is generally accompanied by gelatin, as in stag's horn, bone, ivory, &c. on the contrary, when carbonate of lime is the hardening substance, as in shells, madrepores and millepores, no gelatin can be discovered; for I have frequently digested these

* JOHN COLLICK, Esq. of St. Martin's Lane.

substances many days in boiling distilled water, (after having reduced them to a coarse powder, that they might present a larger surface,) but I never could, by any test, discover the slightest vestige of gelatin. The horns therefore which were first mentioned, are very different from the composition of stag's horn, and yield gradually, and with great difficulty, only a small quantity of gelatin.

Horny scale was next examined; but I shall first here request permission to make a digression in respect to the scales of fish, which I had not examined when my paper on shell and bone was read.

As the scales of fish, when viewed by a microscope, and according to the observations of Mr. LEEUWENHOEK, appear to be formed of different membranaceous laminæ, and as they exhibit the colour and lustre of mother of pearl, it might be expected, that they should prove to be of a similar nature with the substance of stratified shells, or, in other terms, that they should consist of membrane and carbonate of lime.

But, when scales perfectly clean, and separated from the skin of different fish, (such as the salmon and carp,) had been immersed during four or five hours in diluted nitric acid, till they became transparent, and perfectly membranaceous; the acid liquor, being then saturated with pure ammoniac, afforded a copious precipitate, which was proved to be phosphate of lime.

The spiculæ of the shark's skin, formerly mentioned, were found to be of a similar composition; and we may therefore regard the spiculæ and scales of fish as true bony substances, in which the membranaceous part is more predominant than in common bone.

I fully ascertained, that the phosphate of lime was afforded by these substances only; for, when the different skins from which these scales and spiculæ had been taken, were separately examined in the like manner, no phosphate of lime was obtained.

In addition to this I must observe, that the silver or pearly hue of pearl, mother of pearl, and of fish-scales, is only assisted and modified by the relative degrees of opacity produced, in mother of pearl and in pearl, by the interposition of the particles of carbonate of lime, and in the scales by phosphate of lime; for this peculiar lustre principally resides in the membranaceous part, and remains with it when the acetous or muriatic acids are employed as menstrua, but is completely destroyed by the nitric acid.

The horny scales of serpents, lizards, and such like animals, differ from the foregoing; as all of those which I have examined, consist merely of the membranaceous or horny substance, in a more or less indurated state, and appear to be devoid of phosphate of lime, as an ossifying matter.

Horny scales in general, (and the scales of the *Manis pentadactyla* may be mentioned as an example,) afford but very slight traces of gelatin after being long boiled in distilled water; and this small portion of gelatin can only be discovered by the tanning principle, and by nitro-muriate of tin, unless a very large quantity of the scales has been employed.

Human nail digested in boiling distilled water during several days, was only softened; and, like quill, afforded a slight cloud, by the addition of nitro-muriate of tin.

Shavings of ox's hoof, when long digested as abovementioned, afforded a liquor which, in like manner, was only made slightly turbid by nitro-muriate of tin.

Nail and hoof, when long boiled, became of a much darker colour.

The horn-like crust which covers certain insects and other animals, was subsequently examined; the experiments were principally made on the plates which covered the body of a large African scorpion, and on the common tortoise-shell of the shops.

The plates taken from the scorpion were not apparently affected, although digested for a long time in boiling distilled water.

The tanning principle produced no alteration, when added to the water; but a faint white cloud appeared, upon the addition of nitro-muriate of tin.

Tortoise-shell, in thin slips and shavings, was digested in a similar manner during three weeks; but it was only slightly softened; and the water, which had acquired a brownish colour, was but little affected, even by nitro-muriate of tin, which however formed a white cloud.*

From some previous circumstances, which need not here be mentioned, I was lastly induced to make some similar experiments on albumen; and, as that of the blood is mixed with gelatin, and with the substance called fibrin by the chemists, which in chemical properties appears to be the same as muscular fibre, and as it is with some difficulty that the albumen can be exactly separated from these substances, I preferred the albumen of eggs, as being pure and unmixed; and, in order that it might be brought into a state in some measure similar to the bodies lately examined, (by which I mean simple inspissation,) I dried it, after coagulation, in a vessel which was

* The crust which covers insects like the scorpion, appears in every respect to be similar to tortoise-shell.

heated to 212° of FAHRENHEIT, till it became perfectly hard, brittle, yellow, and semi-transparent, like horn.

The albumen, in this state, was digested during eight days in boiling distilled water, which was occasionally renewed, in proportion to the evaporation.

In a few hours after the commencement of the digestion, the transparent horny pieces of albumen were softened, and became white and opaque, exactly like albumen recently coagulated; but, after this, no farther change was observed.

At the end of eight days, the water in which the albumen had been digested was examined, and was found exactly to resemble that afforded by quill, nail, and tortoise-shell; for the transparency of it was not disturbed by the tanning principle, although nitro-muriate of tin produced a faint white cloud.*

As far, therefore, as could be ascertained, by long digestion in boiling distilled water, and by the effects of the re-agents, albumen was proved to be very similar to tortoise-shell, and many of the other substances previously noticed; but the close resemblance, or rather indeed identity, of albumen with those bodies, will be placed in a stronger light, by the enumeration and comparison of their other chemical properties.

As I have, in the former part of this paper, had occasion to mention the gelatin obtained from the Sponges and *Gorgoniæ*, it is not necessary here to repeat those remarks, neither is it requisite that I should enter into any minute account concerning

* When infusion of oak-bark is added to recent liquid albumen, it immediately forms a precipitate; and nitro-muriate of tin does not produce any effect till some hours have elapsed. But after coagulation the reverse takes place; for the water in which coagulated albumen has been long boiled, becomes turbid by the addition of nitro-muriate of tin; and is not in any manner affected by infusion of oak-bark.

the experiments made on bladder, and some other membranes. It may therefore suffice here to observe, that all these bodies afforded more or less gelatin; that, when this was separated, the remaining substance ceased to be tough, or elastic, and was easily torn, like wetted paper; and that, when dry, the sponges, and such membranes as bladder and cuticle, became very brittle, and were shrivelled and curled up, like withered leaves of plants.

But, before I speak of the nature of the substance which thus remained, it will be proper, concisely to notice the effects of acids on the bodies which afford gelatin; and, as the most remarkable effects were produced by nitric acid, I shall to that confine the present observations.

The specific gravity of the nitric acid which I employed in the whole of my experiments, was 1,38; and this acid was diluted with 2, 3, or 4 measures of distilled water, according to the quality of the substance under examination, and the intended time of immersion. But, as an acid too powerful would have frustrated my intentions, I commonly added the acid, by degrees, and at long intervals, to the water in which the substance was immersed; during which time, if any nitrous gas was discharged, more water was added, as this gas was a certain sign that the acid was not sufficiently diluted.

Substances like the *Corallina officinalis*, which contain a large quantity of animal mucilage, or of the least viscid jelly, soon impart it to boiling water.

In like manner, when such substances were steeped in nitric acid diluted with about three measures of water, the mucilage was in a few hours completely dissolved, while the membranaceous part remained untouched.

Pure isinglass dissolved in the diluted nitric acid, formed a

pale yellow liquor, which by evaporation became of a deeper colour, and, when nearly dry, was suddenly reduced to a spongy coal. This change was rapid; and was attended with a considerable effervescence, and a copious discharge of nitrous gas, not unfrequently accompanied by sparks, and sometimes flame; arising undoubtedly from nitrate of ammoniac, which was formed towards the end of the evaporation.

The acid solutions of mucilage, isinglass, and pure glue, were changed to a deeper yellow, when saturated by the absorbent earths, by the alkalies, and especially by pure ammoniac. In such cases, little or no precipitate was obtained from pure gelatinous substances; but some faint traces of phosphoric acid were discovered in these solutions.

The effects of the dilute nitric acid on the other various substances which have been mentioned, resembled those now described, and kept pace exactly with those of boiling water; for, when they were immersed in equal quantities of the dilute acid during a given time, the solution of the gelatin took place according to the order observed in those substances, when water was employed.

As an instance of this, two pieces of skin, recently taken from an ox, were subjected to experiment, as follows:

One of the pieces was boiled in water, till the whole of the cutis was dissolved; after which, the cuticle remained, although very feeble in texture, while the hair did not seem to have suffered any material alteration.

The other piece was steeped in nitric acid diluted with about four measures of distilled water. At the end of five days the cutis was dissolved, and the cuticle was become of a loose and feeble texture; but the hair had not suffered any apparent change,

excepting that of being slightly tinged with yellow. In both cases, therefore, the effects of boiling water and of acid were similar, and were evidently most powerful on those parts which were the most gelatinous.

As water dissolves mucilage more speedily than size, and this last more readily than strong viscid glue, so are the effects of very dilute nitric acid on the same substances; and, when equal quantities of dried mucilage, of eel-skin glue, and of the strongest glue, were dissolved in equal quantities of the dilute acid, the colour of the solutions was more intense, and the change produced by ammoniac was more visible, according to the order of solubility and of tenacity.

It is well known how readily gelatin is dissolved by the caustic fixed alkalies: when, therefore, the varieties of jelly or glue were added to boiling lixivium of caustic potash, they were soon dissolved; and, if added to saturation, a brownish viscid substance was formed.

I did not observe that any ammoniac was produced, neither was any coal deposited, after long boiling the solution in which there was an excess of alkali.

The viscid matter thus obtained, *did not possess the properties of animal soap*; for it neither formed a permanent lather, when mixed and shaken with water; nor, when saturated with acids, did it afford any precipitate; contrary to what happens, when animal soap is thus treated. But, if the gelatinous substance was not pure; if, for example, any parts of membrane, which are not soluble in water, were present, then, in proportion to the quantity of this substance, the alkaline solution exhibited more or less of the saponaceous characters; but these I never observed when pure gelatin was employed.

Gelatin, according to its quantity and quality, has a powerful influence on some of the physical and chemical properties of the bodies in which it is present; by these properties, I mean flexibility, elasticity, and putrescibility.

So much has been said already, in various parts of this paper, tending to prove how much the degrees of flexibility and elasticity, in various animal substances, depend on their gelatinous part, that little need be added; and, when it is considered, that bodies such as muscular fibre, membrane, sponge, hair, and cuticle, being deprived of gelatin, and dried in the air, become rigid and brittle, no doubt can be entertained but that this arises from the loss of the gelatinous substance; and, as an additional proof, when bodies such as nail, feather, quill, and tortoise-shell (which contain little or no gelatin) are long boiled, and then dried in the air, like the former, they are found to have suffered scarcely any alteration in their respective degrees of flexibility and elasticity.

As to putrefaction, it is obvious to every one, that certain parts of animals are much more susceptible of it than others; and that, when the carcase of an animal begins to putrefy, the most humid and flexible parts are always first affected.

Thus, the viscera, muscles, and cutis, soon suffer a change; while hair, feather, scale, horn, hoof, and nail, remain unchanged, ages after the former have been decomposed; and this is evidently caused by the gelatin and moisture, which are combined in the former, and not in the latter, at least in any notable quantity. I have already mentioned the progressive and comparative effects of boiling water, and of dilute nitric acid, on the skin of the ox; and I have shewed, that while the cutis was completely dissolved, the hair remained untouched. These effects

are to be observed, in the same exact order, when a similar piece of skin is exposed to putrefaction; for this commences in, and chiefly affects, the cutis, while the hair is separated, unchanged in its quality. I do not, therefore, hesitate to assert, that the degree of putrescibility in the various parts of animals, depends principally on the presence, and on the quantity and quality, of gelatin; and the skin of the rhinoceros found on the banks of the Vilui, near Yakutsk, was preserved, in all probability, partly by the nature of the climate and soil, and partly by the superior horny quality which it possessed over other skins; for it may be much questioned, whether the hide of an ox or horse, in the same situation, would have escaped putrefaction for so long a period.*

From the preceding observations it appears, that gelatin is a component part of many animal substances.

That it differs in quality, from a very attenuated jelly or mucilage, to that viscid substance called glue; the varieties of which also differ in solubility and tenacity.

That it is present in various proportions; so that certain bodies, such as the cutis, and the cartilages of the joints, are formed by it; while others, like nail, quill, and tortoise-shell, can scarcely be said to contain it.

And that, by its presence, in various states and proportions, it may be regarded (including inherent moisture and organic

* The more viscid gelatinous substances do not appear to be so immediately susceptible of putrefaction as those of the opposite quality; for, when solutions, in water, of animal mucilage, eel-skin glue, and strong glue, were during a certain time exposed under equal circumstances, I found the mucilage to be the first, and the glue the last, which shewed symptoms of putrefaction.

arrangement) as the principal cause of those degrees of flexibility, of elasticity, and of putrescibility, so various in the different parts of animals.*

But, when gelatin has been separated from the different substances, either by repeated boiling with water, or by being steeped in dilute acids, a more insoluble substance remains, of a very different nature, which I shall now proceed to examine.

When a bone or piece of ivory has, by long boiling in

* As gelatin, according to its proportion and quality, appears to produce considerable effects on the parts of animals in which it is present; and, as the gelatin in animal bodies is, in all probability, liable to be changed and modified by morbid causes, it is much to be wished, that gentlemen of the medical profession would ascertain, by experiments, how far the tonic properties of barks depend on the tanning principle.

Mr. BIGGIN has proved, (Phil. Trans. for 1799. p. 259) that willow bark, and especially that of the Huntingdon or Leicester willow, contains the tanning matter in a considerable quantity; and that the latter, in this respect, even equals, or rather exceeds, that of oak.

My friend, the Rev. THOMAS RACKETT, Rector of Spetisbury and Charlton, in Dorsetshire, has employed, in those parishes, the bark of the common willow with great success, as a tonic and febrifuge. Moreover, Mr. WESTRING, of Norrköping, has observed, (*Annales de Chimie*. Tom. XXXI[I]. p. 179.) that those species of *Cinchona* which contain the tanning principle in the greatest quantity, are the most efficacious in fevers; and that the *Cinchona floribunda*, which contains scarcely any tanning matter, is destitute of the abovementioned beneficial effects. Mr. WESTRING therefore, with great apparent reason, believes, that the relative effects produced by the different species of *Cinchona*, when employed in medicine, are in proportion to their tanning power, or the quantity of tanning principle contained in them.

If any one should be induced to make experiments on the tonic effects of the tanning principle, it is to be hoped that some attention would also be paid to the medicinal properties of nitro-muriate of tin, of which, at present, I believe little or nothing is known.

water, been deprived of a great part of its gelatin, and is afterwards steeped in a dilute acid, the ossifying substance is dissolved, and the cartilage remains, retaining the figure of the original bone; or, if a similar bone or piece of ivory, which has not been boiled, is steeped in a dilute acid, (especially nitric acid,) the ossifying substance is dissolved, and, at the same time, but more slowly, the gelatin is separated, and causes the liquor to become yellow, when the phosphate of lime is precipitated by ammoniac.

The cartilaginous body which remains, after the gelatin has been thus separated, is not easily soluble in dilute acids, for (according to its texture) many weeks, and even months, may elapse, before a small part is taken up; but, in concentrated nitric acid, or in boiling dilute acid, it is rapidly dissolved, as I shall soon have occasion to mention.

This substance, when dry, is semi-transparent, like horn, and more or less brittle.

It is the predominant and essential part, in the tissue or web of membrane, cartilage, sponge, the horny stems of *Gorgoniæ*, horn, hair, feather, quill, hoof, nail, horny scale, crust, and tortoise-shell; and, although of similar chemical properties, yet in consistency it varies, from a tender jelly-like substance, to a completely formed membrane, or to an elastic, brittle, and hard body, like tortoise-shell.*

* These bodies, especially tortoise-shell, appear to be formed (as far as organic arrangement is concerned) in the way of *stratum super stratum*. This structure is peculiarly to be discovered after long maceration in diluted nitric acid; for then, tortoise-shell appears to be composed (like the black polished *Gorgonia*) of membranaceous laminæ; and the varieties of horn differ only by a tendency to the fibrous organization.

Experiments were made separately, on each of the bodies above enumerated; but, as I did not find any essential difference in the results, I shall include the whole under the following observations.

1. When distilled, a small portion of water, some carbonate of ammoniac, a fœtid empyreumatic oil, carbonated hydrogen gas, carbonic acid gas, and prussic acid, were obtained.

2. A spongy coal, of a gray metallic lustre, remained: this, by incineration, afforded a very small residuum, which was not always similar in quantity, even in portions of the same substance; for, 500 grains of tortoise-shell, taken from different samples, afforded from $\frac{1}{4}$ of a grain to 3 grains of residuum, which consisted of phosphate of lime, and phosphate of soda; sometimes also a little carbonate of lime was present; but I do not believe these to be essential ingredients.

3. When boiled many days in distilled water, the substance was softened; and the water became slightly turbid with nitro-muriate of tin; but no effect was produced by the tanning principle.

4. Muriatic and sulphuric acids had little effect, unless heated; and the same was the case with nitric acid much diluted, or in the state proper to extract and separate gelatin; but, if the immersion in the dilute acid was continued during some weeks, the acid gradually acquired a yellow tinge, and, when saturated with ammoniac, became of a deeper colour, without having its transparency disturbed.

5. The substance which had thus been long steeped in the acid, was much softened, was become more transparent, and, from being horny, was now more like a cartilaginous substance: when taken out of the acid, if it was immediately

steeped in pure ammoniac, it changed to a deep orange colour, inclining to blood red; it was gradually and silently dissolved, without any residuum, and a deep orange or yellowish brown coloured liquor was formed.

6. Or, when taken out of the acid, if it was first well washed in distilled water, and then boiled, it was also dissolved, and formed a pale yellowish solution: this, by evaporation and cooling, became a jelly, which was again soluble in boiling water; and was precipitated, like gelatin, by the tanning principle, and, more slowly, by nitro-muriate of tin.

7. If the nitric acid in which the substance was immersed was not sufficiently diluted, or if heat was applied, the whole was rapidly dissolved, with a considerable effervescence, and discharge of nitrous gas.

8. This solution was yellow, like the former, the colour being intense, in proportion to the quantity dissolved; and it was also changed to a deep orange or yellowish brown by the addition of ammoniac, without depositing any precipitate, unless a large quantity had been dissolved.

9. The nitric solutions of this substance, when evaporated, afforded much the same appearances as those of gelatin, but the coal which remained was less spongy.

10. This substance (whether of sponge, horn, quill, hair, nail, or tortoise-shell, &c.) was strongly distinguished from gelatin, by the effects produced when boiled with caustic fixed alkali; for animal soaps were formed, exactly similar in every property excepting colour, and the whole of the original substance was completely dissolved.

11. During the process, a considerable quantity of ammoniac

was discharged; and, if the alkali was in excess, some coal was deposited.

12. When the animal soap was dissolved, diluted with distilled water, and filtrated, if an acid (such as the acetous, or muriatic) was added, a copious precipitate was obtained, which was re-dissolved by an excess of acid.

13. This precipitate, being collected upon a filter, appeared at first like a yellow or brownish viscid substance, which, when dry, was like a thick coat of varnish, or dried white of egg, and in like manner was brittle, and broke with a glossy fracture.

14. It burned like quill or tortoise-shell, leaving a spongy coal; and, when distilled, afforded products like those obtained from the bodies abovementioned.

15. It was not readily soluble in dilute acids; and was acted upon by nitric acid and ammoniac like the substances from which it had been obtained; the properties also of its solutions in nitric acid and ammoniac were similar.

16. With caustic lixivium of potash it readily combined, and again formed animal soap.

17. It was not quite so insoluble in boiling water as quill or tortoise-shell; and the water in which it had been boiled was not only made turbid by nitro-muriate of tin, but yielded a precipitate when infusion of oak-bark was added, after the manner of gelatin.

These experiments proved, that this precipitate was the same as the original substance from which it had been obtained; and that the only change it had suffered, was that of being rendered rather more soluble in boiling water.

The whole series of experiments on the various bodies lately

enumerated, convinced me moreover, by the similarity of results, that they essentially consisted of one and the same substance, modified in texture by the degrees of organic arrangement, and by the occasional presence, and different proportion and quality, of gelatin.

But the difference in chemical properties shewed, that this last mentioned substance (I mean gelatin) was quite distinct from that which is now under examination; and, from the resemblance of certain effects observed when quill and tortoise-shell were compared with inspissated albumen, by being long digested in boiling water, I was induced to make a series of comparative experiments upon albumen, similar in every respect to those which have been so lately described, of which the following are the results.

1. By distillation, the coagulated dry semi-transparent albumen, afforded products exactly similar to those obtained from tortoise-shell, and the other substances which have just been examined.

2. A spongy coal remained, of very difficult incineration; as towards the end of the process it appeared vitrified, and glazed with a melted saline coat, which was, however, easily dissolved by water.

The residuum was again exposed to a long continued red heat, and again treated with water, till, at length, a few scarcely visible particles remained; which, as far as such a small quantity would permit to be ascertained, proved to be phosphate of lime.

The portion dissolved by water (which was by much the most considerable) consisted principally of carbonate, mixed with a small quantity of phosphate of soda.

3. When steeped in dilute nitric acid, it was not soon affected;

but, after about four weeks, the acid began to be tinged with yellow, which gradually became deeper in the course of three months; the albumen, however, although less transparent, was but little diminished.

The yellow acid solution, when saturated with ammoniac, changed to a deep orange colour, and remained transparent.

4. The albumen which had been thus steeped in the dilute nitric acid, was immediately immersed in ammoniac; which changed it to a deep orange colour, inclining to a blood red, and gradually dissolved it, without any apparent residuum.

This solution was of a deep yellowish brown.

5. If the albumen, instead of being immersed in ammoniac, was washed, and then boiled with distilled water, it was dissolved, and formed a pale yellow liquor, which, by evaporation, formed a gelatinous mass; this, being dissolved again in boiling water, was (like gelatin) precipitated by the tanning principle, and more slowly by nitro-muriate of tin.

6. By concentrated nitric acid, or by the dilute acid when heated, albumen was speedily dissolved, with much effervescence, and a copious discharge of nitrous gas.

7. This solution was like that of tortoise-shell, and the other substances mentioned in the former experiments.

8. When evaporated, it afforded similar results.

9. Albumen, like tortoise-shell, quill, and nail, was dissolved by boiling lixivium of caustic potash, and formed animal soap.

10. In like manner also, a considerable portion of ammoniac was discharged; and, if the alkali was in excess, some coal was deposited.

11. The animal soap obtained from albumen, when dissolved

in water, yielded a precipitate, by the addition of acetous or muriatic acid; and the precipitate was re-dissolved, when the acid was added to excess.

12. This precipitate, when collected on a filter, appeared more saponaceous, and less viscid, than that obtained from the substances lately examined.* When gently heated, some oil flowed from it; after which, a brownish viscid substance remained, similar in its properties to that which was obtained from the animal soap made by tortoise-shell, and the other bodies.†

13. It may not be improper here to repeat, that inspissated albumen, long boiled with distilled water, was not apparently diminished; but the water (like that in which tortoise-shell, quill, or nail, has been boiled) had acquired the property of becoming white and turbid, when nitro-muriate of tin was added, although it was not changed by the tanning principle.

To this also may be added, that the water in which tortoise-shell, nail, and albumen, had been boiled, became in some measure putrid in a few days, and emitted an offensive smell.

I am not inclined, however, to regard this as a proof that any gelatin had been separated from these bodies by means of boiling water, but rather that inspissated albumen, tortoise-shell, *&c.*

* This precipitate, when obtained from different substances, such as hair, wool, and muscular fibre, appeared in some cases more, and in others less viscid, although similar in every other property.

It will be proper also here to observe, that the saponaceous solutions were always filtrated, before the addition of the acids.

† The yolk of eggs, when boiled with caustic lixivium of potash, forms a pale olive-coloured concrete animal soap, which, when dissolved in water, and saturated with muriatic acid, is precipitated in the state of mere fat.

Yolk of egg, by incineration, affords a small portion of phosphate of soda and of lime.

are substances really soluble, although in so slight a degree as to approach insolubility; and that thus the prevalent opinion has arisen concerning the insolubility of coagulated albumen in boiling water.

Neither is the putrefaction of the water in which the bodies abovementioned have been boiled, a proof that any other than their real substance has been dissolved; for this putrefaction appears to depend on its attenuated and diluted state, more than on any other cause. Tortoise-shell, nail, quill, and similar bodies, certainly are not liable to putrefaction; neither is albumen, when in the inspissated semi-transparent state.

This last substance also, when merely coagulated, does not easily putrefy; for I kept it, when it was soft, white, and coagulated, in water, during the whole of the month of last April, without finding that it became really putrid: towards the latter part of the time, it had rather a disagreeable smell; still, however, it was far from being absolutely putrid.

But albumen which has not been coagulated, or which has been diluted and shaken with a quantity of cold water, begins in a very few days to be putrid; liquid albumen, therefore, enters easily into putrefaction, although it is the reverse with that which is dense and solid: and, from a comparison of the preceding experiments upon tortoise-shell, quill, nail, *&c.* with those made on albumen, I am induced to believe, that the former bodies are essentially composed of albumen, modified by the various effects of organization, and reduced to a state of density far exceeding that which is produced by simple inspissation.

And, although the bodies, which of late have been particularly mentioned, appear to consist principally of albumen, with

sometimes the addition of gelatin in different proportions, yet, as in certain membranes and such like substances, portions of muscular fibre were at times found joined or interwoven; and as muscle, ligament, and tendon, seem to glide almost imperceptibly into each other, I was almost unavoidably induced to make some experiments on muscular fibre.

The muscular fibre on which the greater part of these experiments was made, was that of beef; and, in order to separate the liquid albuminous part or lymph as much as possible, a quantity of lean muscle of ox beef, cut into small thin pieces, was macerated during 15 days in cold water, and was subjected to pressure each day, when the water was changed. The weather was very cold; and the maceration was continued to the end of the 15th day, without any sign of putrescency.

The shreds of muscle (amounting to about 3 pounds) were then boiled with about 6 quarts of water, during 5 hours; and, the water being changed, the same was repeated every day, during the course of three weeks; at the end of which time, the water afforded only slight signs of gelatin, when infusion of oak-bark, or nitro-muriate of tin, was added. After this, the fibrous part was well pressed, and was dried by the heat of a water bath.

Some of the muscular fibre thus prepared, was steeped in nitric acid diluted with three measures of water, during 15 days. The acid acquired a yellow tinge, and possessed all the properties of the nitric solutions of albumen.

The fibre which had been thus steeped in the acid, was (when washed) dissolved by boiling water, and by evaporation became a gelatinous mass; which, being again dissolved in boiling water, was precipitated by infusion of oak-bark, and,

more slowly, by nitro-muriate of tin, like the albuminous substances, when treated in a similar manner.

When the fibre which had been steeped in the acid was immersed in ammoniac, it was not completely dissolved, like albumen, but afforded a residuum, which will soon be noticed. The greater part was, however, thus dissolved; and formed a deep orange or yellowish brown solution, similar in properties to that of albumen.

When boiled with lixivium of caustic potash, this muscular fibre was completely dissolved; ammoniac was discharged, and animal soap was formed; which, being diluted with water, and saturated with muriatic acid, yielded a precipitate, similar in every property to that which had been obtained from the animal soaps formerly mentioned, excepting, that it sooner became hard and glossy, when exposed to the air.*

Muscular fibre, when prepared as already mentioned, so as (by long maceration, and subsequent boiling, with frequent change of water) to be very nearly deprived of the whole of its gelatinous part, is not easily brought into the putrid state. A small quantity was kept moistened with water, during the whole of last April; in the course of which time, it acquired a musty but not a putrid smell; neither were the fibres reduced

* In respect to œconomical purposes, it may be proper here to observe, that all animal substances whatever, (exclusive of carbonate and phosphate of lime,) may be converted into two substances of much utility, namely, glue, (under which term I include all the varieties mentioned in this paper,) and soap; with the additional advantage, that those parts which would be rejected in making the one, are the most proper to prepare the other.

The offensive smell of CHAPTAL'S soap is considered as an objection; but this may be removed, by exposing the soap for some time, in flat vessels, to the air; after which, it may be reduced to the proper degree of consistency, by a second boiling.

to a pulpy mass.* I am inclined therefore to suspect, that strong and completely formed muscular fibre, considered as a distinct substance, is not of easy putrescibility; and that the readiness with which muscle in general enters into putrefaction, is principally owing to the gelatin, which is combined and mixed with it, in a large proportion, as a component part, and which, with the natural quantity of moisture, is requisite to give the fibre a proper degree of toughness and flexibility.

The residuum afforded by muscular fibre which had been long steeped in dilute nitric acid, and afterwards immersed in ammoniac, consisted principally of fat, mixed with a small portion of the fibre which had not been sufficiently acted upon by the acid; and little or no earthy matter was thus obtained.

But, when the prepared muscular fibre was dissolved in boiling nitric acid, a complete solution, resembling that of albumen, in its general properties, was formed; and some fat floated, in drops, at the top of the liquor.

Ammoniac was then added, so as to super-saturate the acid, and produced the same effects as on the nitric solutions of albumen, excepting, that a copious white precipitate was obtained.

This precipitate, while moist, was agitated with a quantity of acetous acid, which dissolved, and separated, a small portion of phosphate of lime; but the remainder, and by much the greatest part of this precipitate, was scarcely attacked, even when the acid was boiled.

When exposed to a red heat, it became dark gray, and then

* A portion of this muscular fibre was kept under water during two months; it did not however become putrid, nor was it converted into that fatty substance which is obtained from recent muscle, under similar circumstances.

nearly white; after which, it was in the state of carbonate of lime.

Another part was dissolved in nitric acid, and lime was precipitated by carbonate of soda. The slight excess of the latter was then saturated by acetous acid; and the whole was boiled, to expel the carbonic acid; after which, the liquor, from its effects on solutions of lime, barytes, *&c.* evidently contained oxalic acid in solution: the precipitate was therefore oxalate of lime, mixed with a very small quantity of phosphate of lime.

200 grains of the dry muscular fibre, dissolved and boiled with nitric acid, afforded 17 grains of this precipitate.

Although it is known that the gelatinous liquor obtained from muscle by boiling water, contains phosphate of soda, and of lime, yet I did not imagine that the greater part of the latter could be so completely separated.

I therefore, in some measure, repeated the experiment, on the muscle of veal; and (as I expected) found phosphate of soda, and of lime, in the liquor. But, when the muscle was afterwards dissolved in boiling nitric acid, and the solution was saturated with ammoniac, I was surprized to find that, although the same change in colour was produced as in all the former experiments, the liquor remained transparent; and, even after several days, only a few scattered particles appeared at the bottom of the vessel.

Another experiment was made, on the recent muscle of mutton; but this was immediately dissolved in nitric acid, without being previously boiled in water. The fat being separated, the solution was, as before, saturated with ammoniac; and, as usual, became of a deep orange colour, or yellowish brown: in a few hours also, a small quantity of a white precipitate subsided.

This precipitate, however, was completely and readily soluble in acetous acid; and in every respect proved to be phosphate of lime.

Before I proceed, it will be proper to observe, that the liquor from which the above precipitate was separated, as well as those afforded by the muscle of veal, by the prepared muscle of beef, by the solutions of tortoise-shell and of albumen, in boiling nitric acid, subsequently saturated by ammoniac, all contained a considerable portion of uncombined oxalic acid, which was separated by acetite of lime, and of lead. But I did not find oxalic acid in the solutions formed by immersing these bodies, for a long time, in cold and dilute nitric acid; neither did I find oxalic acid in solutions made by dissolving these substances in boiling muriatic acid. It is evident, therefore, that the oxalic acid observed in the above experiments, was a product of the operations, and not an educt of the substances.

We may conclude, from the experiments on the muscular substances which have been lately mentioned, that they contain lime, in various proportions, and in two different states, viz. carbonate and phosphate; and that the greater part of the latter is gradually separated, in conjunction with the gelatin, by means of boiling water. I would not however have it understood, that phosphate of lime is an essential ingredient in gelatinous substances; for, on the contrary, isinglass, which is a perfectly gelatinous body, affords but a mere visible trace of it. The muscular fibre of beef appears to have been nearly deprived of its phosphate of lime, by the long continued and repeated boiling in water to which it had been subjected; but still so large a quantity of lime remained, that when oxalic acid was formed by the action of the boiling

nitric acid, it combined with the lime, and formed an oxalate, which amounted to 17 grains, from 200 grains of the dry muscular fibre, dissolved in nitric acid, and precipitated by ammoniac.

I do not know what quantity of phosphate of lime was separated with the gelatin, as I was then only intent on preparing the fibrous part of the muscle; but, from the quantity of lime which remained, and which afterwards combined with the oxalic acid, it is evident, that in the muscle of beef there is a considerable portion of earthy matter; and as, by the experiment on the muscle of veal, scarcely any precipitate was obtained after it had been boiled, and as but a small portion of phosphate of lime was present in the gelatinous liquid, it appears that, in this muscle, the whole of the small portion of lime which it contained was in the state of phosphate; and this being nearly separated, there did not remain any part of uncombined lime, or carbonate of lime, which, by uniting with the oxalic acid, (subsequently produced,) would form an oxalate; and as lime, in the states of phosphate and carbonate, is so much more abundant in the muscle of beef than in that of veal, we may infer, that the earthy matter is more abundant in the coarse and rigid fibre of adult and aged animals, than in the tender fibre of those which are young; and this seems to be corroborated, by the tendency to morbid ossification, so frequently observed in aged individuals of the human species.

Gelatin, albumen, and muscular fibre, not only differ very much from each other by the relative quantity of their saline or earthy residua, but also by the proportion of one of their essential and elementary principles, namely, carbon. 500 grains of isinglass, made perfectly dry by distillation, yielded 56 grains

of coal, from which, 1,50 grains of earthy residuum, obtained by incineration, being deducted, the proportion of coal appears to have been 54,50 grains.

500 grains of dry albumen, afforded 74,50 grains; and, as the saline residuum amounted to 11,25 grains, the quantity of mere coal was 63,25 grains.

500 grains of tortoise-shell, yielded 80 grains of coal; from which, 3 grains of earthy matter being deducted, 77 grains remain, for the proportion of coal.

And 500 grains of the dry prepared muscular fibre of beef, when distilled, left 108 grains of coal, which, by incineration, afforded 25,60 grains of earthy residuum; the coal may therefore be estimated at 82,40 grains.

There appears much reason therefore to believe, that the gelatinous substances and muscular fibre, differ from simple and unorganized albumen, by a diminution of the carbonic principle in the one, and by an excess of it in the other; and as, in vegetables, the fibrous part is that which contains the largest proportion of carbon, so, in respect to the other animal substances, muscular fibre appears to contain the greatest quantity of it.

The nature of the residua obtained by the incineration of the substances lately mentioned, also deserves to be noticed.

Only 1,50 grain was obtained from 500 grains of isinglass; and, as far as the quantity would allow, was proved to be phosphate of soda, mixed with a very minute proportion of phosphate of lime.

The 3 grains afforded by tortoise-shell, consisted of phosphate of soda and of lime, with some traces of iron: it is probable, that the latter was accidentally present.

The prepared muscular fibre of beef yielded 25,60 grains; the greatest part of which was carbonate of lime, mixed with some pure lime, and a small portion of phosphate: there can be no doubt but that the latter would have been more abundant, had it not been for the repeated boilings to which the muscular fibre had been subjected.

The recent muscles of veal and mutton were with great difficulty reduced to ashes; for, towards the end of the process, the ashes and remaining coal became coated and glazed with saline matter, which appeared to be soda, partly in the state of phosphate; and it is not a little remarkable, that the 11,25 grains obtained from albumen, consisted principally of soda, in a caustic state, (by reason of the long continued heat,) mixed with a small quantity of phosphate of soda, and a very minute portion of phosphate of lime.

Pure albumen, therefore, which has not been subjected to the effects of organization, appears to contain a considerable portion of saline matter, and very little of any earthy substance; but the contrary seems to happen, in bodies which (although evidently derived from albumen) have suffered various changes by the action of the vital principle; which may be considered as the cause of organization, by which these bodies are differently modified, according to the nature of the parts of animals which, singly or conjointly, they are employed to form. In these bodies, the quantity of the saline substances appears to be diminished, while that of the earthy matter is increased, especially in the coarser kinds of muscular fibre.

Upon a comparison of the chemical properties of the substance which remains, after the separation of gelatin from the great variety of animal substances which have been so often

mentioned in the course of this paper, and which need not therefore now be repeated, there can scarcely be any doubt but that it is one and the same substance, in different states of density and texture.

For the similarity of its nature was proved by,

1st. The effects of fire, and the products obtained by distillation.

2dly. Its very difficult solubility by long digestion in boiling water.

3dly. The effects produced by re-agents, on the water in which bodies like inspissated albumen or tortoise-shell had been boiled.

4thly. The effects of acids, (particularly nitric acid,) of ammoniac, and of caustic lixivium of potash.

5thly. The animal soap which was formed; and the precipitate obtained from it, by the addition of acetous or muriatic acid.* And,

6thly. The difficulty attending the putrefaction of the substance in question, when pure and dense.

The similarity in all these properties, appears to me a full proof, that it is the same substance which constitutes the principal part of membrane, sponge, horn, hair, *&c.* and even of muscular fibre.

Moreover, upon comparing the properties of this substance with those of pure albumen in a state of inspissation, so evident a resemblance in every respect is discovered, that few I believe will hesitate to pronounce albumen to be the original substance from which tortoise-shell, hair, horn, muscular fibre, *&c.* have been derived and formed.

* This appears to be a strong marked character of the albuminous substances.

There is also much reason to believe that gelatin, although it appears so different in many respects from albumen, is yet formed from it.*

It may be recollected, that in a former part of this paper mention was made, that tortoise-shell, horn, muscular fibre, and inspissated albumen, after long immersion in very dilute nitric acid, and after being well washed, were soluble in boiling water; and that a substance was formed, which (by becoming liquefied when heated, by being soluble in boiling water, by being precipitated by the tanning principle and by nitro-muriate of tin, and lastly, by forming a gelatinous mass when the aqueous solution

* In addition to the chemical properties by which gelatin and albumen are distinguished, particularly the different effects observed when these two substances were treated with nitric acid, I shall mention some others, not less remarkable, which are produced by the muriatic acid.

When any of the varieties of gelatin, such as glue, isinglass, &c. are immersed in cold muriatic acid, they are dissolved in a few hours; and the solutions thus formed suffer no apparent change, even in the course of several months. In like manner, gelatin may be separated and dissolved from bodies which contain it, such as sponge, bladder, skin, and muscle; but the part which remains undissolved, and which, with the other substances formerly mentioned, I regard as formed of albumen more or less organized, is very differently affected; for, when coagulated albumen, the undissolved part of bladder, muscular fibre, feather, quill, tortoise-shell, wool, and hair, were separately steeped in muriatic acid, they gradually became of a dark colour, and the acid was tinged with the same. The colour afforded by albumen was deep blue, inclining to purple; that of bladder was brownish purple; feather, quill, tortoise-shell, and muscular fibre, afforded a beautiful deep blue; and wool and hair, like bladder, produced a brownish purple. The change began to take place in the coagulated albumen in about eight or ten days; but wool and hair were the last which were affected. In about three months, the different liquids were become very dark, although scarcely any perceptible quantity of the immersed substances appeared to be dissolved. Nitric acid, in a small proportion, changed these blue and brownish purple liquors to deep yellow; and ammoniac, being then added, changed them to orange colour, and produced all those effects which were observed, when the nitric solutions of these substances were thus treated.

was sufficiently evaporated and cooled,) approached and resembled gelatin.

It would be perhaps too hasty to assert, that gelatin was thus absolutely formed; but, if a substance so very similar to it could be thus produced, we may with some reason conclude, that the real gelatin, with its various modifications, is formed from albumen, by the more efficacious and delicate operations of nature.

In attempting to prove that albumen or the coagulating lymph is the original animal substance, I have hitherto only stated chemical facts; but, when the phænomena attending incubation are considered; when the experiments made by eminent physiologists, such as HALLER, MAITRE JEAN, and MALPIGHI, are viewed; when the oviparous fœtus is seen to be progressively formed in and from the albumen of the egg, so that, upon the bursting of the shell which separated it from external matter, the young animal comes forth complete in all its parts; when such strong facts as these are corroborated by those afforded by chemistry, it can scarcely be doubted that albumen is the primary animal substance, from which the others are derived; and there is much cause to believe that the formation of gelatin, and of the animal fibre especially, begins with the process of sanguification in the fœtus.

As the three principal and essential component parts of the blood, *viz.* albumen, gelatin, and fibre,* appear therefore to

* The whole of the blood, which by anatomists is divided into serum, red globules, and coagulating lymph, when chemically examined, is found to consist of albumen, gelatin, and fibre. The serum which remains liquid after the coagulation of the blood, is composed of albumen, gelatin, some saline matter, and much water.

The clot or crassamentum also affords, by repeated washing, a large proportion of

compose the various parts of animals, in such a manner that one (being predominant) influences the nature of that part of the animal which it is principally employed to form; and as albumen, gelatin, and fibre, by relative proportion, by the degrees of density, by the effects of organization which singly or conjointly they have experienced, by the texture of the animal substance which they, as materials and thus modified, have concurred to produce, and by the proportion of natural or inherent moisture peculiar to each part of different animals, present an immense series of complicated causes, so are the effects found to be no less numerous and diversified, by the infinite variety in texture, flexibility, elasticity, and the many other properties peculiar to the various parts which compose the bodies of animals.

albumen and gelatin; after which, a substance remains, in appearance very analogous to muscular fibre, excepting that it is in a more attenuated state. This substance (called fibrin by chemists) may be regarded as that part of the blood which has undergone the most complete animalization; and from which the muscular fibre and other organs of the body are formed.

XVII. *On the Electricity excited by the mere Contact of conducting Substances of different kinds. In a Letter from Mr.* Alexander Volta, *F. R. S. Professor of Natural Philosophy in the University of Pavia, to the Rt. Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read June 26, 1800.

A Côme en Milanois, ce 20me Mars, 1800.

Après un long silence, dont je ne chercherai pas à m'excuser, j'ai le plaisir de vous communiquer, Monsieur, et par votre moyen à la Société Royale, quelques résultats frappants auxquels je suis arrivé, en poursuivant mes expériences sur l'électricité excitée par le simple contact mutuel des métaux de différente espèce, et même par celui des autres conducteurs, aussi différents entr'eux, soit liquides, soit contenant quelque humeur, à laquelle ils doivent proprement leur pouvoir conducteur. Le principal de ces resultats, et qui comprend à-peu-près tous les autres, est la construction d'un appareil qui ressemble pour les effets, c'est-à-dire, pour les commotions qu'il est capable de faire éprouver dans les bras, &c. aux bouteilles de Leyde, et mieux encore aux batteries électriques foiblement chargées, qui agiroient cependant sans cesse, ou dont la charge, après chaque explosion, se rétabliroit d'elle-même; qui jouiroit, en un mot, d'une charge indéfectible, d'une action sur le fluide électrique, ou impulsion, perpétuelle; mais qui d'ailleurs en diffère essentiellement, et

par cette action continuelle qui lui est propre, et parcequ'au lieu de consister, commes les bouteilles et batteries électriques ordinaires, en une ou plusieurs lames isolantes, en couches minces de ces corps censés être les seuls *électriques*, armées de conducteurs ou corps ainsi dit *non-électriques*, ce nouvel appareil est formé uniquement de plusieurs de ces derniers corps, choisis même entre les meilleurs conducteurs, et par là les plus éloignés, suivant ce qu'on a toûjours cru, de la nature électrique. Oui, l'appareil dont je vous parle, et qui vous étonnera sans doute, n'est que l'assemblage d'un nombre de bons conducteurs de différente espèce, arrangés d'une certaine manière. 30, 40, 60 pièces, ou d'avantage, de cuivre, ou mieux d'argent, appliquées chacune à une pièce d'étain, ou, ce qui est beaucoup mieux, de zinc, et un nombre égal de couches d'eau, ou de quelque autre humeur qui soit meilleur conducteur que l'eau simple, comme l'eau salée, la lessive, &c. ou des morceaux de carton, de peau, &c. bien imbibés de ces humeurs; de telles couches interposées à chaque couple ou combinaison des deux métaux différents, une telle suite alternative, et toûjours dans le même ordre, de ces trois espèces de conducteurs, voilà tout ce qui constitue mon nouvel instrument; qui imite, comme j'ai dit, les effets des bouteilles de Leyde, ou des batteries électriques, en donnant les mêmes commotions que celles-ci; qui, à la vérité, reste beaucoup au-dessous de l'activité des dites batteries chargées à un haut point, quant à la force et au bruit des explosions, à l'étincelle, à la distance à laquelle peut s'opérer la décharge, &c. egalant seulement les effets d'une batterie chargée à un degré très-foible, d'une batterie pourtant ayant une capacité immense; mais qui d'ailleurs surpasse infiniment la vertu et le pouvoir de ces mêmes batteries, en ce qu'il n'a pas besoin, comme elles,

d'être chargé d'avance, au moyen d'une électricité étrangère; et en ce qu'il est capable de donner la commotion, toutes les fois qu'on le touche convenablement, quelques fréquents que soient ces attouchements.

Cet appareil, semblable dans le fond, comme je ferai voir, et même tel que je viens de le construire, pour la forme, à l'*organe électrique naturel* de la torpille, de l'anguille tremblante, &c. bien plus qu'à la bouteille de Leyde, et aux batteries électriques connues, je voudrois l'appeller *Organe électrique artificiel*. Et au vrai n'est-il pas, comme celui-là, composé uniquement de corps conducteurs? n'est-il pas au surplus actif par lui-même, sans aucune charge précédente, sans le sécours d'une électricité quelconque excitée par aucun des moyens connus jusqu'ici; agissant sans cesse, et sans relâche; capable enfin de donner à tout moment des commotions plus ou moins fortes, selon les circonstances, des commotions qui redoublent à chaque attouchement, et qui, repétées ainsi avec fréquence, ou continuées pour un certain tems, produisent ce même engourdissement des membres que fait éprouver la torpille, &c.?

Je vais vous donner ici une description plus détaillée de cet appareil, et de quelques autres analogues, aussi bien que des expériences relatives les plus remarquables.

Je me fournis de quelques douzaines de petites plaques rondes ou disques, de cuivre, de laiton, ou mieux d'argent, d'un pouce de diamétre, plus ou moins, (par exemple, de monnoyes,) et d'un nombre égal de plaques d'étain, ou, ce qui est beaucoup mieux, de zinc, de la même figure et grandeur, à-peu-près; je dis à-peu-près, parcequ'une précision n'est point requise, et, en général, la grandeur, aussi bien que la figure, des pièces métalliques, est arbitraire: on doit avoir égard seulement qu'on puisse les

arranger commodément les unes sur les autres, en forme de colonne. Je prépare en outre, un nombre assez grand de rouelles de carton, de peau, ou de quelque autre matière spongieuse, capable d'imbiber et de retenir beaucoup de l'eau, ou de l'humeur dont il faudra, pour le succès des expériences, qu'elles soient bien trempées. Ces tranches ou rouelles, que j'appellerai disques mouillés, je les fais un peu plus petites que les disques ou plateaux métalliques, afin qu'interposées à ceux, de la manière que je dirai tantôt, ils n'en débordent pas.

Ayant sous ma main toutes ces pièces, en bon état, c'est-à-dire, les disques métalliques bien propres et secs, et les autres non-métalliques bien imbibés d'eau simple, ou, ce qui est beaucoup mieux, d'eau salée, et essuyés ensuite légèrement, pour que l'humeur n'en dégoutte pas, je n'ai plus qu'à les arranger comme il convient; et cet arrangement est simple et facile.

Je pose donc horizontalement sur une table ou base quelconque, un des plateaux métalliques, par exemple, un d'argent, et sur ce premier j'en adapte un second de zinc; sur ce second je couche un des disques mouillés; puis un autre plateau d'argent, suivi immédiatement d'un autre de zinc, auquel je fais succéder encore un disque mouillé. Je continue ainsi, de la même façon, accouplant un plateau d'argent avec un de zinc, et toûjours dans le même sens, c'est-à-dire, toûjours l'argent dessous et le zinc dessus, ou *vice versâ*, selon que j'ai commencé, et interposant à chacun de ces couples, un disque mouillé; je continue, dis-je, à former, de plusieurs de ces étages, une colonne aussi haute qu'elle peut se soutenir sans s'écrouler.

Or, si elle parvient à contenir environ 20 de ces étages ou couples de métaux, elle sera déjà capable, non seulement de faire donner des signes à l'électrometre de CAVALLO, aidé du

condensateur, au-delà de 10 ou 15 dégrés, de charger ce condensateur par un simple attouchement, au point de lui faire donner une étincelle, &c. mais aussi de frapper les doigts avec lesquels on vient toucher ses deux extremités, (la tête et le pied d'une telle colonne,) d'un ou de plusieurs petits coups, et plus ou moins fréquents, selon qu'on réitère ces contacts; chacun desquels coups ressemble parfaitement à cette légère commotion que fait éprouver une bouteille de Leyde foiblement chargée, ou une batterie chargée beaucoup plus foiblement encore, ou enfin une torpille extrêmement languissante, qui imite encore mieux les effets de mon appareil, par la suite des coups répétés qu'elle peut donner sans cesse.

Pour obtenir de telles légères commotions de cet appareil que je viens de décrire, et qui est encore trop petit pour de grands effets, il est nécessaire que les doigts avec lesquels on veut toucher ses deux extremités en même tems, soient humectés d'eau, au point que la peau, qui autrement n'est pas un assez bon conducteur, se trouve bien trempée. Encore, pour réussir plus sûrement, et recevoir des commotions considérablement plus fortes, faut-il faire communiquer, par le moyen d'une lame suffisamment large, ou d'un gros fil métallique, le pied de la colonne, c'est-à-dire, le plateau du fond, avec l'eau d'un bassin ou coupe assez grande, dans laquelle on tiendra plongé un doigt, deux, trois, ou toute la main, tandis qu'on ira toucher la tête ou extrémité supérieure, (le dernier ou un des derniers plateaux de cette colonne,) avec l'extrémité nette d'une lame aussi métallique, empoignée par l'autre main, qui doit être bien humide, et embrasser une large surface de cette lame, et la serrer fortement. En procédant de cette manière, je puis déjà obtenir un petit picotement, ou légère commotion, dans une ou deux

articulations d'un doigt plongé dans l'eau du bassin, en touchant, avec la lame empoignée dans l'autre main, la quatrième, ou même la troisième paire de plateaux; touchant ensuite la cinquième, la sixième, et de proche en proche les autres, jusqu' au dernier plateau, qui fait la tête de la colonne, il est curieux d'éprouver comment les commotions augmentent graduellement en force. Or, cette force est telle, que je parviens à recevoir d'une telle colonne, formée de 20 paires de plateaux, (pas davantage,) des commotions qui prennent tout le doigt, et l'affectent même assez douloureusement, s'il est plongé seul dans l'eau du bassin; qui s'étendent (sans douleur) jusqu'au poignet, et même jusqu' au coude, si la main est plongée en grande partie, ou entièrement, et se font sentir encore au poignet de l'autre main.

Je suppose toujours qu'on ait pratiqué toutes les attentions nécessaires dans la construction de la colonne, que chacune des paires ou couples de métaux, resultant d'une plaque d'argent appliquée à une de zinc, se trouve en communication avec la couple suivante, par une couche suffisante d'humeur, qui soit de l'eau salée, plutôt que de l'eau pure, ou par un disque de carton, de peau, ou autre chose semblable, bien imbibée de cette eau salée; lequel disque ne soit pas trop petit, et dont les surfaces soient bien collées aux surfaces des plateaux métalliques, entre lesquels il se trouve interposé. Cette application exacte et étendue des disques mouillés, est très-importante; au lieu que les plateaux métalliques de chaque paire, peuvent ne se toucher entr'eux qu'en peu de points, pourvu seulement que leur contact soit immédiat.

Tout cela fait voir (pour le dire ici en passant) que si le contact des métaux entr'eux en quelques points seulement suffit (étant tous d'excellents conducteurs) pour donner libre passage à un courant électrique médiocrement fort, il n'en est pas de

même pour les liquides, ou pour les corps imbibés d'humeur, qui sont des conducteurs beaucoup moins parfaits, et qui, par conséquent, ont besoin d'un ample contact avec les conducteurs métalliques, et plus encore entr'eux, pour que le fluide électrique puisse passer avec assez de facilité, et pour qu'il ne soit pas trop retardé dans son cours, sur-tout lorsqu'il est mu avec très-peu de force, comme dans notre cas.

Au reste, les effets de mon appareil (les commotions qu'on éprouve) sont considérablement plus sensibles, à mesure que la température de l'air ambient, ou celle de l'eau, ou des disques mouillés qui entrent dans la composition de la colonne, et de l'eau même du bassin, est plus chaude; la chaleur rendant l'eau plus conductrice. Mais, ce qui la rend beaucoup meilleure encore, ce sont presque tous les sels, et notamment le sel commun. Voilà une des raisons, sinon la seule, pourquoi il est si avantageux que l'eau du bassin, et sur-tout celle interposée à chaque paire de plateaux métalliques, l'eau dont sont imbibés les disques de carton, &c. soit de l'eau salée, comme j'ai déjà fait remarquer.

Mais tous ces moyens, et toutes ces attentions, enfin, n'ont qu'un avantage limité, et ne feront jamais qu'on puisse obtenir des commotions bien fortes, tant que l'appareil ne consistera qu'en une seule colonne formée de 20 paires seulement de plateaux, quoiqu'ils soient des deux meilleurs métaux pour ces expériences, sçavoir, d'argent et de zinc; car, s'ils étoient d'argent et de plomb, ou d'étain, ou de cuivre et d'étain, on n'obtiendroit pas la moitié de l'effet, à moins qu'un nombre beaucoup plus grand ne suppléât à la moindre force de chaque paire. Or donc, ce qui augmente réellement la puissance électrique de cet appareil, et la peut porter au degré d'égaler, et de surpasser encore, celle de la torpille et de l'anguille tremblante, c'est le

nombre des plateaux, arrangés de la manière, et avec les attentions, que j'ai expliqué. Si, aux 20 paires décrites ci-dessus, on en ajoute 20 ou 30 autres, disposées dans le même ordre, les commotions que pourra donner la colonne ainsi prolongée, (je dirai tantôt comment on peut la soutenir, pour qu'elle ne s'écroule pas, ou, ce qui est mieux, la partager en deux ou plusieurs colonnes,) seront déjà beaucoup plus fortes, et s'étendront dans les deux bras jusqu'à l'épaule, sur-tout dans celui dont la main est plongée dans l'eau; laquelle main, avec le bras entier, en restera plus ou moins engourdie, si, en réitérant les attouchements avec fréquence, on fait succéder ces commotions l'une à l'autre rapidement, et sans relâche. Cela, en plongeant toute, ou presque toute, la main dans l'eau du bassin; mais, si on ne plonge qu'un doigt seul, en tout ou en partie, les commotions concentrées presque dans lui seul, en seront d'autant plus douloureuses, et si cuisantes qu'elles deviendront insupportables.

On s'attend bien que cette colonne, formée de 40 ou 50 couples de métaux, qui donne des commotions plus que médiocres aux deux bras d'une personne, pourra en donner encore de sensibles à plusieurs, qui, se tenant par leurs mains, (suffisamment humides,) forment une chaine non interrompue.

Revenant à la construction mecanique de mon appareil, qui est susceptible de plusieurs variations, je vais décrire ici, non pas toutes celles que j'ai imaginées et exécutées, soit en grand, soit en petit, mais quelques unes seulement, qui sont ou plus curieuses, ou plus utiles; qui présentent quelqu' avantage réel, comme d'être d'une exécution plus facile, ou plus expéditive, d'être plus immanquables dans leurs effets, ou plus long-tems conservables en bon état.

Et pour commencer par une, qui, réunissant à-peu-près tous

ces avantages, diffère le plus, quant à sa figure, de *l'appareil à colonne* décrit ci-dessus, mais qui a le désavantage d'être une machine beaucoup plus volumineuse; je vous présente ce nouvel appareil, que j'appellerai *à couronne de tasses*, dans la figure ci-jointe. (Pl. XVII. Fig. 1.)

On dispose donc une rangée de plusieurs tasses ou coupes, de quelque matière que ce soit, exceptés les métaux, de tasses de bois, d'écaille, de terre, ou mieux de cristal, (des petits verres à boire ou gobelets, sont les plus à-propos,) à demi pleines d'eau pure, ou mieux d'eau salée, ou de lessive; et on les fait communiquer toutes, on en forme une espèce de chaine, par le moyen d'autant d'arcs metalliques, dont un bras A *a*, ou seulement l'extrémité A, qui plonge dans un des gobelets, est de cuivre rouge, ou jaune, ou mieux de cuivre argenté, et l'autre Z, qui plonge dans le gobelet suivant, est d'étain, ou mieux de zinc. J'observerai ici, en passant, que la lessive et les autres liqueurs alcalines sont préférables, lorsqu' un des métaux qui doivent plonger, est l'étain; l'eau salée est préférable, lorsque c'est le zinc. Les deux métaux dont chaque arc se compose, sont soudés ensemble, dans quelque endroit que ce soit, au-dessus de la partie qui plonge dans le liquide, et qui doit le toucher par une surface suffisamment large: il est pour cela convenable, que cette partie soit une lame d'un pouce quarré, ou très-peu moins; le reste de l'arc peut être plus étroit tant qu'on veut, et même un simple fil métallique. Il peut aussi être d'un troisième métal, différent des deux qui plongent dans le liquide des gobelets; puisque l'action sur le fluide électrique, qui résulte de tous les contacts de plusieurs métaux qui se succédent immédiatement, la force avec laquelle ce fluide se trouve poussé à la fin, est la même absolument, ou à-peu-près, que celle qu'il auroit

reçu par le contact immediat du premier métal avec le dernier, sans aucun des métaux intermédiaires, comme j'ai verifié par des expériences directes, dont j'aurai occasion de parler ailleurs.

Or donc, une suite de 30, 40, 60, de ces gobelets, enchainés de cette manière, et rangés, soit dans une ligne droite, soit dans une courbe, ou repliée de toutes les manières, forme tout ce nouvel appareil; qui dans le fond, et en substance, est le même que l'autre à colonne, décrit plus haut; l'essentiel, qui consiste dans la communication immediate des métaux différents qui forment chaque couple, et médiate d'une couple avec l'autre, savoir, par l'interméde d'un conducteur humide, ayant lieu pour l'un, aussi bien que pour l'autre de ces appareils.

Quant à la manière de mettre celui à gobelets à l'épreuve, et quant aux différentes expériences auxquelles il peut servir, je n'ai pas besoin d'en dire beaucoup, après ce que j'ai fait observer, et expliqué amplement, au sujet de l'autre à colonne. On comprendra aisément, que pour avoir la commotion, il suffit de plonger une main dans un des gobelets, et un doigt de l'autre main dans un autre gobelet, assez eloigné de celui-la; que cette commotion sera d'autant plus forte que ces deux vases seront plus éloignés l'un de l'autre, c'est-à-dire, qu'il y en aura un plus grand nombre d'intermédiaires; que, par conséquent, on aura la plus forte, en touchant le premier et le dernier de la chaine. On comprendra aussi comment, et pourquoi, les expériences réussiront beaucoup mieux, en empoignant, et serrant, dans une main bien humectée, une lame métallique assez large, (afin que la communication soit ici assez parfaite, et se fasse par un grand nombre de points,) et touchant avec cette lame l'eau du gobelet, ou plutôt l'arc métallique designé, tandis que l'autre main se trouve plongée dans l'autre gobelet éloigné, ou touche,

avec une lame empoignée de même, l'arc de celui-ci. Enfin on comprendra, et on pourra même prévoir le succès d'une grande variété d'expériences, qu'on peut exécuter avec cet *appareil à couronne de tasses*, plus facilement, et d'une manière plus évidente et parlante, pour ainsi dire, aux yeux, qu'avec l'autre appareil à colonne. Je me dispenserai donc de décrire un grand nombre de ces expériences faciles à deviner, et j'en rapporterai seulement quelques unes, qui ne sont pas moins instructives qu'amusantes.

Soient trois vingtaines de ces tasses ou gobelets, rangés et enchaînés l'un à l'autre par les arcs métalliques, mais de façon que, pour la première vingtaine, ces arcs soient tournés dans le même sens, par exemple, le bras d'argent tourné à gauche, et le bras de zinc à droite; et pour la seconde vingtaine, en sens contraire, c'est-à-dire, le zinc à gauche, et l'argent à droite; enfin, pour la troisième vingtaine, de nouveau, l'argent à gauche, comme pour la première. Ces choses ainsi disposées, plongez un doigt dans l'eau du premier gobelet, et touchez, avec la lame empoignée par l'autre main, de la manière prescrite, le premier arc métallique, (celui qui joint le premier gobelet au second,) puis l'autre arc qui embrasse le second et le troisième gobelet, et successivement les autres arcs, jusqu'à les parcourir tous. Si l'eau est bien salée et tiède, et la peau des mains assez humectée et ramollie, vous commencerez déjà à éprouver une petite commotion dans le doigt, lorsque vous serez parvenu à toucher le 4e ou le 5e arc; (je l'ai éprouvée quelquefois assez distinctement par le contact du 3e;) et, en passant successivement au 6e, 7e, &c. les secousses augmenteront graduellement de force, jusqu'au 20e arc, c'est-à-dire, jusqu'au dernier de ceux tournés dans le même sens: mais, en passant outre, au 21e, 22e, 23e, ou 1er, 2d, 3e,

de la seconde vingtaine, dans laquelle ils sont tous tournés en sens contraire, les secousses deviendront à chaque pas moins fortes, si bien, qu'au 36e, ou 37e, elles seront imperceptibles, et absolument nulles au 40e; passé lequel, (et commençant la troisième vingtaine, opposée à la seconde, et analogue à la première,) les secousses seront encore imperceptibles, jusqu'au 44e ou 45e arc; mais elles recommenceront à dévenir sensibles, et à augmenter graduellement, à mesure que vous avancerez, jusqu'au 60e, où elles seront arrivées à la même force du 20e arc.

Or, si les 20 arcs du milieu étoient tournés dans le même sens que les 20 précedents et les 20 suivants, si tous les 60 conspiroient à pousser le fluide électrique dans la même direction, on comprend de combien l'effet seroit plus grand à la fin, et la commotion plus forte; et en général on comprend comment, et jusqu'à quel point, elle doit être affoiblie, dans tous les cas ou un nombre plus ou moins grand de ces forces, par la position des métaux à l'opposite, se contrarient.

Si la chaine est interrompue quelque part, soit que l'eau manque dans une des tasses, soit qu'un des arcs métalliques ait été enlevé, ou qu'il soit séparé en deux pieces, vous n'aurez aucune commotion en plongeant un doigt dans l'eau du premier, et un autre dans l'eau du dernier vase; mais vous l'aurez, forte ou foible, selon les circonstances, (laissant ces doigts plongés,) au moment qu'on rétablira la communication rompue, au moment qu'une autre personne plongera dans les deux tasses où manque l'arc, deux de ses doigts, (qui seront aussi frappés d'une légère commotion,) ou mieux, qu'elle y plongera ce même arc qu'on avoit ôté, ou un autre quelconque; et, dans le cas de l'arc separé en deux piéces, au moment qu'on ramenera celles-ci au contact mutuel; (de laquelle manière la commotion sera

plus forte qu'autrement;) enfin, dans le cas de la tasse vuide, au moment qu'en y versant de l'eau, elle abordera aux deux bras métalliques enfoncés dans cette tasse, et qui se trouvoient à sec.

Lorsque la chaine ou couronne de tasses est assez longue, et en état de pouvoir donner une forte commotion, on l'éprouvera, quoique beaucoup plus foible, quand même on tiendroit plongés les deux doigts, ou les deux mains, dans un seul bassin d'eau assez grand, dans lequel aboutissent le premier et le dernier arc métallique, pourvu que l'une ou l'autre de ces mains enfoncées, ou mieux toutes les deux, on les tienne respectivement en contact de ces mêmes arcs, ou assez près du contact; on éprouvera, dis-je, une commotion, au moment que (la chaine se trouvant interrompue quelque part) la communication sera rétablie, et le cercle completé, d'une des manières qu'on vient de dire. Or, on pourroit être surpris, que dans ce cercle, le courant électrique, ayant son passage libre à travers une masse d'eau non interrompue, dans cette eau qui remplit le bassin, quitte ce bon conducteur, pour se jetter, et poursuivre son cours, à travers le corps de la personne qui tient ses mains plongées dans cette même eau, en faisant ainsi un plus long trajet. Mais la surprise cessera, si on reflechit, que les substances animales vivantes et chaudes, et sur-tout leurs humeurs, sont en général des meilleurs conducteurs que l'eau. Le corps donc de la personne qui plonge les mains dans l'eau, offrant un passage plus facile que cette eau au torrent électrique, celui-ci doit le préférer, quoiqu'un peu plus long. Au reste, comme le fluide électrique, lorsqu'il doit traverser en quantité, des conducteurs qui ne sont pas parfaits, et nommément des conducteurs humides, aime à s'étendre dans un canal plus large, ou à se partager en plusieurs,

et à prendre même des détours, trouvant en cela moins de résistance qu'à suivre un seul canal, quoique plus court; ce n'est dans notre cas qu'une partie du torrent électrique, qui, s'écartant de l'eau, prend cette nouvelle route de la personne, et la parcourt d'un bras à l'autre: une autre partie, plus ou moins grande, passe à travers l'eau du bassin. Voilà la raison pourquoi la secousse qu'on éprouve, est beaucoup plus foible que lorsque le courant électrique n'est point partagé, lorsque la personne fait seule la communication d'un arc à l'autre, &c.

D'après ces expériences, on peut croire, que lorsque la torpille veut donner une secousse aux bras de l'homme, ou aux animaux qui la touchent, ou qui s'approchent de son corps sous l'eau, (laquelle secousse est pareillement beaucoup plus foible que celle que le poisson peut donner hors de l'eau,) elle n'a qu'à rapprocher quelques unes des parties de son organe électrique, là où, par quelque intervalle, la communication manque; qu'à ôter ces interruptions entre l'une et l'autre des colonnes dont est formé le dit organe, ou entre ces membranes, en forme de disques minces, qui gissent les unes sur les autres, du fond jusqu'au sommet de chaque colonne; elle n'a, dis-je, qu'à ôter ces interruptions dans un ou plusieurs endroits, et y faire naître le contact convenable, soit en comprimant ces mêmes colonnes, soit en faisant couler entre les pellicules ou diaphragmes soulévés, quelqu'humeur, &c. Voilà quelle peut être, et, comme j'imagine, quelle est réellement, toute la tâche de la torpille, en donnant la commotion; car tout le reste, je veux dire l'incitation et mouvement donné au fluide électrique, n'est qu'un effet nécessaire de son organe singulier, formé, comme on voit, d'une suite très-nombreuse de conducteurs, que j'ai tout le fondement de croire assez différents entr'eux pour être aussi

moteurs du fluide électrique, dans leurs contacts mutuels, et de les supposer arrangés de la manière convenable pour pousser ce fluide avec une force suffisante, de haut en bas, ou de bas en haut, et déterminer un courant capable de produire la commotion, &c. sitôt, et chaque fois, que tous les contacts et communications nécessaires ont lieu.

Mais laissons maintenant la torpille, et son *organe électrique naturel*, et revenons à l'*organe électrique artificiel* de mon invention, et particulièrement à celui qui imite le premier, même par la forme, (car celui à gobelets s'en éloigne à cet égard,) revenons à mon premier *appareil à colonne*. J'aurois quelque chose encore à dire par rapport à la construction du dit appareil à gobelets ou *à couronne de tasses*, par exemple, qu'il est bon que la première et la dernière tasse soient assez grandes pour pouvoir y plonger, à l'occasion, toute la main, &c.; mais il seroit trop long d'entrer dans tous ces détails.

Quant à l'appareil à colonne, j'ai cherché les moyens de l'allonger beaucoup, en multipliant les plateaux métalliques sans qu'elle s'écroulât; de rendre cet instrument commode et portatif, et, sur-tout, durable; et j'ai trouvé, entr'autres, les suivants, que je vous mets sous les yeux, par les figures ci-jointes. (Pl. XVII. Fig. 2, 3, 4.)

Dans la Fig. 2de, *m m m m*, sont des montants ou baguettes, au nombre, de trois, quatre, ou plus, qui s'élèvent du pied de la colonne, et renferment, comme dans un cage, les plateaux ou disques posés les uns sur les autres, en tel nombre, et jusqu'à la hauteur qu'on veut, et les empêchent ainsi de tomber. Les baguettes peuvent être de verre, de bois, ou de métal; seulement, dans ce dernier cas, il faut empêcher qu'elles touchent immédiatement les plateaux; ce qu'on peut faire, ou en couvrant

chacune de ces baguettes métalliques avec un tube de verre, ou en interposant entre celles-ci et la colonne, quelques bandes de toile cirée, de papier huilé, ou même de papier simple, ou tout autre corps enfin, qui soit ou *cobibent*, ou mauvais conducteur: le bois, ou le papier, le sont assez pour notre cas, pourvu seulement qu'ils ne soient pas extrêmement humides, ou mouillés.

Mais le meilleur expédient, lorsqu'on veut former l'appareil d'un nombre très-grand de plateaux, au-delà, par exemple, de 60, 80, 100, est de partager la colonne en deux ou plusieurs, comme on voit dans les Figures 3 et 4; (Pl. XVII.) où les pièces ont toutes leurs positions et communications respectives, comme si c'étoit une seule colonne. On peut en effet regarder la Fig. 4e, aussi bien que la 3e, comme une colonne repliée.

Dans toutes ces figures, les plateaux métalliques différents sont designés par les lettres A et Z; (qui sont les initielles d'*argent* et de *zinc;*) et les *disques mouillés* (de carton, de peau, &c.) interposés à chaque couple de ces métaux, par une couche noire. Les lignes ponctuées marquent l'union d'un métal avec l'autre, dans chaque couple, leur contact mutuel par un nombre quelconque de points; ce qui est indifférent, ou qu'ils sont soudés ensemble, ce qui est bien à plus d'un égard. *c c*, *c c*, *c c*, sont des plaques métalliques, qui font communiquer une colonne, ou section de colonne, à l'autre; et *b*, *b*, *b*, *b*, *b*, sont, les bassins d'eaux, en communication avec les pieds ou extrémités des colonnes.

Un appareil ainsi monté est assez commode, pas volumineux, et on pourroit le rendre encore plus facilement et plus sûrement portatif, à l'aide de quelques étuis ou canons, dans lesquels on enfermeroit et garderoit chaque colonne. C'est dommage seulement qu'il ne dure pas long-tems en bon état; les disques

mouillés se desséchant, dans un ou deux jours, au point qu'il faut les humecter de nouveau; ce qu'on peut faire pourtant, sans démonter tout l'appareil, en plongeant les colonnes toutes faites dans l'eau, et (les ayant retirées quelques tems après) les essuyant à l'extérieur avec un linge, ou autrement, le mieux qu'on peut.

La meilleure manière d'en faire un instrument aussi durable qu'on peut le souhaiter, seroit d'enfermer et retenir l'eau interposée à chaque couple de métaux, et de fixer ces mêmes plateaux à leurs places, en enveloppant de cire ou de poix toute la colonne; mais la chose est un peu difficile pour l'exécution, et exige beaucoup de patience. J'y ai pourtant réussi; et j'ai formé, de cette manière, deux cilindres de 20 couples métalliques, qui me servent encore assez bien, après quelques semaines, et serviront, j'espère, après des mois.

On a la commodité de pouvoir employer ces cilindres aux expériences, non seulement debout, mais inclinés, ou couchés, comme on veut, et même plongés dans l'eau, la tête seulement dehors: ils pourroient encore donner la commotion plongés entièrement, s'ils contenoient un nombre plus grand de plateaux, ou si plusieurs de ces cilindres étoient joints ensemble, et qu'il y eût quelqu'interruption, qu'on pût ôter à volonté, &c. avec quoi, ces cilindres imiteroient assez bien l'anguille tremblante; pour mieux ressembler à laquelle, même dans l'extérieur, ils pourroient être joints ensemble par des fils métalliques pliables, ou des ressorts à boudin, et être couverts dans toute la longueur d'une peau, et se terminer en une tête et en une queue, bien configurées, &c.

Les effets sensibles à nos organes que produit un appareil formé de 40 ou 50 paires de plateaux, (et même un moins grand, si l'un des métaux étant argent ou cuivre, l'autre est zinc,) ne

se reduisent pas simplement aux commotions: le courant de fluide électrique, mu et sollicité par un tel nombre et espèces de conducteurs différents, argent, zinc, et eau, alternativement disposés de la manière décrite, n'excite pas seulement des contractions et spasmes dans les muscles, des convulsions plus ou moins violentes dans les membres qu'il traverse dans son cours, mais il irrite aussi les organes du goût, de la vue, de l'ouïe, et du tact, proprement dit, et y produit des sensations propres à chacun.

Et, premièrement, quant au sens du tact; si, au moyen d'un ample contact de la main (bien humectée) avec une lame métallique, ou mieux, en plongeant la main profondément dans l'eau du bassin, j'établis d'un côté une bonne communication avec une des extrémités de mon *appareil électro-moteur*, (il faut donner de nouveaux noms à des instruments nouveaux, non seulement par la forme, mais aussi par les effets, ou par le principe d'où ils dependent,) et de l'autre côté j'applique le front, la paupière, le bout du nez, aussi humectés, ou quelque autre partie du corps où la peau soit assez délicate; j'applique, dis-je, avec un peu de pression, quelqu'une de ces parties delicates, bien humectées, contre la pointe d'un fil métallique, qui va communiquer convenablement à l'autre extrémité du dit appareil, je sens, au moment que s'accomplit ainsi le cercle conducteur, à l'endroit touché de la peau, et un peu au-delà, un coup et une piqûre, qui passent vite, et se repètent autant de fois qu'on interrompt et rétablit ce cercle; de sorte que, si ces alternatives sont fréquentes, elles me causent un trémoussement, et un picotement fort désagréable. Mais, si toutes les communications continuent sans ces alternatives, sans la moindre interruption du cercle, je ne ressens plus rien pour quelques moments; passés lesquels, commence à la partie appliquée au bout du fil métallique, une

autre sensation, qui est une douleur aigue, (sans secousse,) limitée précisément aux points du contact, une cuisson, non seulement continuée, mais qui va toûjours en augmentant, au point de devenir en peu de tems insupportable, et qui ne cesse qu'en interrompant le cercle.

Quelle preuve plus évidente de la continuation du courant électrique, pour tout le tems que les communications des conducteurs qui forme le cercle continuent ? et que seulement en interrompant celui-ci, un tel courant est suspendu ? Cette circulation sans fin du fluide électrique, (ce *mouvement perpetuel,*) peut paroitre paradoxe, peut n'être pas explicable; mais elle n'en est pas moins vraie et réelle, et on la touche, pour ainsi dire, des mains. Une autre preuve evidente peut aussi se tirer, de ce que, dans ces sortes d'expériences, on éprouve souvent, au moment qu'on interrompt brusquement le cercle, un coup, une piqûre, une commotion, suivant les circonstances, tout comme au moment qu'on le complete; avec la seule différence, que ces sensations, causées par une espèce de reflux du fluide électrique, ou par le secousse qui naît de la suspension soudaine de son courant, sont plus foibles. Mais je n'ai pas besoin, et ce n'est pas ici le lieu, d'alléguer les preuves d'une telle circulation sans fin du fluide électrique, dans un cercle de conducteurs où il y en a qui, pour être de différente espèce, font par leur contact mutuel, l'office d'excitateurs ou *moteurs :* cette proposition, que j'ai avancée dès mes premières recherches et découvertes au sujet du GALVANISME, et toûjours soutenue, en l'appuyant de nouveaux faits et expériences, n'aura plus, j'espére, de contradicteurs.

Revenant à la sensation de douleur qu'on éprouve dans les expériences décrites ci-dessus, je dois ajouter, que si cette dou-

leur est assez forte et piquante dans les parties que la peau recouvre, elle l'est beaucoup plus où la peau a été enlévé, dans les blessures, par exemple, et les plaies recentes. Si par hazard il y a une petite incision, ou écorchure, au doigt que je plonge dans l'eau communiquante avec une des extrémités de l'appareil électro-moteur, j'y ressens une douleur si vive et si cuisante, lorsqu'en établissant la communication convenable avec l'autre extrémité j'en complete le cercle, que je dois bientôt me désister de l'expérience, c'est-à-dire, retirer le doigt, ou interrompre de quelque autre manière ce cercle. Je dirai de plus, que je ne puis pas même resister au-delà de quelques secondes, lorsque la partie de l'appareil que je mets en jeu, ou l'appareil entier, ne va qu'à 20 couples métalliques, ou environ.

Une chose que je dois encore faire remarquer, c'est, que toutes ces sensations de picotement et de douleur sont plus fortes et plus aigues, les autres choses égales, lorsque la partie du corps qui doit les ressentir se trouve du côté de l'électricité négative, c'est-à-dire, placée de manière, dans le cercle conducteur, que le fluide électrique parcourant ce cercle, ne soit pas dirigé contre cette partie sensible, qu'il ne s'avance pas vers elle et y entre de dehors en dedans, mais bien que sa direction soit de dedans en dehors, en un mot, qu'il en sorte : par rapport à quoi, il faut connoître, des deux métaux qui entrent par couples dans l'appareil construit, quel est celui qui donne à l'autre. Or, j'avois déjà determiné cela pour tous les métaux, par d'autres expériences, publiées il y a long-tems, à la suite de mes premiers mémoires au sujet du GALVANISME. Je ne dirai donc ici autre chose, sinon que tout est pleinement confirmé, par les expériences également et encore plus démonstratives et éclatantes, qui m'occupent à présent.

Par rapport au sens du goût, j'avois déjà découvert, et publié dans ces premiers mémoires, où je me vis obligé de combattre la prétendue électricité animale de GALVANI, et de la déclarer une électricité extrinséque, mue par le contact mutuel des métaux de différente espèce; j'avois, dis-je, découvert, en conséquence de ce pouvoir que j'attribuois aux métaux, que deux piéces de ces métaux différents, et singulièrement une d'argent et une de zinc, appliquées convenablement, excitoient, sur le bout de la langue, de sensations de saveur très-marquées; que la saveur etoit decidément acide, si, le bout de la langue étant tourné vers le zinc, le courant électrique alloit contre lui, et entroit; et qu'une autre saveur, moins forte, mais plus désagréable, acre, et tirante à l'alcalin, se faisoit sentir, si (la position des métaux étant renversée) le courant électrique sortoit du bout de la langue; que ces sensations, au surplus, continuoient, et recevoient même des accroissements, pendant plusieurs secondes, si le contact mutuel des deux métaux se soutenoit, et le cercle conducteur n'étoit nulle part interrompu. Or, quand j'ai dit ici, que les mêmes phénomènes arrivent ponctuellement, lorsqu'on met à l'épreuve, au lieu d'une seule couple de ces piéces métalliques, un assemblage de plusieurs, arrangés comme il faut; et que les dites sensations de saveur, soit acide, soit alcaline, augmentent, mais peu, avec le nombre de ces couples, j'ai presque tout dit. Il me reste seulement à ajouter, que si l'appareil qu'on met en jeu pour ces expériences sur la langue, est formé d'un nombre assez grand de couples métalliques de cette espèce, si, par exemple, il en contient 30, 40, ou davantage, la langue n'éprouve pas uniquement la sensation de saveur qu'on vient de dire, mais, en outre, celle d'un coup, qui la frappe á l'instant qu'on complete le cercle, et qui lui cause une piqûre plus

ou moins douloureuse, mais passagère, suivie, quelques moments après, de la sensation durable de saveur. Ce coup produit même une convulsion, ou trémoussement, d'une partie, ou de toute la langue, lorsque l'appareil, formé d'un plus grand nombre encore de couples des dits métaux, est plus actif, et que, moyennant de bonnes communications conductrices, le courant électrique qu'il excite peut passer partout, avec assez de liberté.

Je reviens souvent, et j'insiste, sur cette dernière condition, parcequ'elle est essentielle, pour toutes les expériences où il s'agit d'obtenir des effets bien sensibles sur notre corps, soit des commotions dans les membres, soit des sensations dans les organes des sens. Il faut donc, que les conducteurs non-métalliques qui entrent dans le cercle, soient des bons conducteurs autant que possible, bien imbibés (s'ils ne sont pas des liquides eux-mêmes) d'eau, ou de quelque autre fluide plus conducteur que l'eau pure; et il faut, outre cela, que les surfaces bien humides, par lesquelles ils communiquent avec les conducteurs métalliques, et sur-tout entr'eux, soient assez larges. La communication doit seulement être retrecie, ou réduite à un petit nombre de points de contact, là où l'on veut concentrer l'action électrique sur une partie des plus sensibles du corps, sur quelques nerf des sens, &c. comme je l'ai déjà fait remarquer, à propos des expériences sur le tact, savoir, des expériences par lesquelles on excite des douleurs aigues dans différentes parties. Ainsi donc, la meilleure manière que j'ai trouvée, de produire sur la langue toutes les sensations décrites, est, d'appliquer son bout contre l'extrémité pointue (qui ne le soit pas pourtant trop) d'une verge métallique, que je fais communiquer convenablement, comme dans les autres expériences, à

une des extrémités de mon appareil, et d'établir un bonne communication de la main, ou, ce qui est mieux, des deux mains ensemble, avec l'autre extrémité. Cette application du bout de la langue au bout de la verge métallique, peut, au reste, ou exister déjà, lorsqu'on va faire l'autre communication pour completer le cercle, (lorsqu'on va plonger la main dans l'eau du bassin,) ou se faire après l'établissement de cette communication, pendant que la main se trouve plongée; et, dans ce dernier cas, je crois sentir la piqûre et la secousse dans la langue, un tant-soit-peu avant le véritable contact. Oui, il me paroit toûjours, particulièrement si j'avance peu-à-peu le bout de la langue, que lorsqu'il est arrivé à une très-petite distance du métal, le fluide électrique, (je voudrois presque dire l'étincelle,) franchissant cet intervalle, s'élance pour le frapper.

A l'égard du sens de la vue, que j'avois aussi découvert pouvoir être affecté par le foible courant du fluide électrique, procédant du contact mutuel de deux métaux differents, en général, et en particulier d'une piéce d'argent avec une de zinc, je devois m'attendre, que la sensation de lumière excitée par mon nouvel appareil, seroit plus forte, à mesure qu'il contiendroit un plus grand nombre de piéces de ces métaux; chaque couple desquels, arrangées comme il faut, ajoute un dégré de force au dit courant électrique, comme toutes les autres expériences le montrent, et notamment celles avec l'électrometre, aidé du condensateur, que j'ai seulement indiquées, et que je décrirai ailleurs. Mais je fus surpris de trouver, qu'avec 10, 20, 30 couples, et davantage, l'éclair produit ne paroissoit ni plus long et étendu, ni beaucoup plus vif, qu'avec une seule couple. Il est vrai, cependant, que cette sensation de lumière foible et passagère, est excitée par un tel appareil plus aisément, et de plusieurs manières. En

effet, pour réussir avec une seule couple, il n'y a, à-peu-près, que les manières suivantes; savoir, ou qu'une des piéces métalliques soit appliquée au bulbe même de l'œil, ou à la paupière, bien humectée, et qu'on la fasse toucher à l'autre métal appliqué à l'autre œil, ou tenu dans la bouche, ce qui donne le plus bel éclair; ou, qu'on empoigne cette second piéce métallique, avec la main bien humectée, et qu'on la porte au contact de la première; ou enfin, qu'on applique ces deux lames à certaines parties de l'intérieur de la bouche, en les faisant aussi communiquer entr'elles. Mais, avec un appareil de 20, 30 couples, &c. on produit le même éclair, en appliquant au bout d'une lame ou verge métallique, qui soit en communication avec une des extremités de cet appareil, tandis que d'une main on communique convenablement avec l'autre extremité; en appliquant, dis-je, ou faisant toucher à cette lame, non seulement l'œil, ou quelque partie que se soit de la bouche, mais le front, le nez, les joues, les levres, le menton, et jusqu'à la gorge; en un mot, toutes les parties et points du visage, qu'on doit seulement avoir bien humectés, avant de les porter au contact de la lame métallique. Au reste, la forme, comme la force, de cette lumière passagère qu'on appercoit, varie un peu, en variant les endroits de la face sur lesquels on porte l'action du courant électrique; si c'est sur le front, par exemple, cette lumière est médiocrement vive, et paroit comme un cercle lumineux, sous laquelle figure elle se présente aussi dans plusieurs autres essais.

Mais la plus curieuse de toutes ces expériences, est de tenir la lame métallique serrée entre les levres, et en contact du bout de la langue; puisque, lorsqu'on vient ensuite completer le cercle, de la manière convenable, on excite à la fois, si l'appareil est suffisamment grand, en bon ordre, et le courant électrique

assez fort et en bon train, une sensation de lumière dans les yeux, une convulsion dans les levres, et même dans la langue, une piqûre douloureuse sur son bout, suivie enfin de la sensation de saveur.

Je n'ai plus qu'à dire un mot sur l'ouie. Ce sens, que j'avois inutilement cherché à exciter avec deux seules lames métalliques, quoique les plus actives entre tous les *moteurs* d'électricité, savoir, une d'argent, ou d'or, et l'autre de zinc, je suis enfin parvenu à l'affecter avec mon nouvel appareil, composé de 30 ou 40 couples de ces métaux. J'ai introduit, bien avant dans les deux oreilles, deux espèces de sondes ou verges métalliques, avec les bouts arrondis; et je les ai fait communiquer immédiatement aux deux extrémités de l'appareil. Au moment que le cercle a été ainsi complété, j'ai reçu une secousse dans la tête; et, quelques moments après, (les communications continuant sans aucune interruption,) j'ai commencé à sentir un son, ou plutôt un bruit, dans les oreilles, que je ne saurois bien définir; c'étoit une espèce de craquement à secousse, ou petillement, comme si quelque pâte ou matière tenace bouillonnoit. Ce bruit continua sans relâche, et sans augmentation, tout le tems que le cercle fut complet, &c. La sensation désagréable, et que je craignis dangereuse, de la secousse dans le cerveau, a fait que je n'ai pas repété plusieurs fois cette expérience.

Reste le sens de l'odorat, que j'ai tenté jusqu'ici inutilement, avec mon appareil. Le fluide électrique, qui, mis en courant dans un cercle complet de conducteurs, produit dans les membres et parties des corps vivants qui se trouvent comprises dans ce cercle, des effets correspondants à leur excitabilité; qui, stimulant particulièrement les organes ou nerfs du tact, du goût, de la vue, et de l'ouie, y excite quelques sensations propres à

chacun de ces sens, comme nous avons trouvé, ne produit, dans l'intérieur du nez, qu'un picotement plus ou moins douloureux, et des commotions plus ou moins étendues, selon que le dit courant est plus ou moins fort. Et d'où vient donc, qu'il n'y excite aucune sensation d'odeur, quoiqu'il arrive, comme il paroit, à stimuler les nerfs de ce sens? On ne peut pas dire, que le fluide électrique, par lui-même, ne soit pas propre à produire des sensations odorantes; puisque, lorsqu'il se repand dans l'air, en forme d'aigrettes, &c. dans les expériences ordinaires des machines électriques, il porte au nez une odeur très-marquée, ressemblante à celle du phosphore. Je dirai donc, avec plus de ressemblance, et sur un fondement d'analogie avec les autres matières odoriférantes, qu'il faut justement qu'il se repande dans l'air, pour exciter l'odorat; qu'il a besoin, comme les autres effluves, du vehicule de l'air, pour affecter ce sens de la manière propre à y faire naître les sensations d'odeur. Or, dans les expériences dont il est question, c'est-à-dire, du courant électrique dans un cercle de conducteurs tous contigus, et sans la moindre interruption, cela ne peut absolument avoir lieu.

Tous les faits que j'ai rapporté dans ce long écrit, touchant l'action que le fluide électrique, incité et mu par mon appareil, exerce sur les différentes parties de notre corps, que son courant envahit et traverse; action qui, au surplus, n'est pas momentanée, mais soutenue et durable pour tout le tems que, les communications n'étant point interrompues, ce courant suit son train; action, enfin, dont les effets varient suivant la différente excitabilité de ces parties, comme on a vu; tous ces faits, déjà assez nombreux, et d'autres qu'on pourra encore découvrir, en multipliant et variant les expériences de ce genre, vont ouvrir un champ assez vaste de reflexions, et des vuës, non seulement

curieuses, mais intéressantes particulièrement la médecine. Il y en aura pour occuper l'anatomiste, le physiologiste, et le practicien.

On sçait, par l'anatomie qui en a été faite, que l'organe électrique de la torpille, et de l'anguille tremblante, consiste en plusieurs colonnes membraneuses, remplies d'un bout à l'autre d'un grand nombres de lames ou pellicules, en forme de disques très-minces, couchées les unes sur les autres, ou soutenues à de très-petits intervalles, dans lesquels coule, comme il paroit, quelque humeur. Or, on ne peut pas supposer, qu'aucune de ces lames soit isolante, comme le verre, les resines, la soye, &c. et moins encore, qu'elles puissent, ou s'électriser par frottement, ou être disposées et chargées à la maniere de petits tableaux FRANKLINIENS, ou de petits électrophores; ni même, qu'elles soient d'assez mauvais conducteurs pour faire l'office d'un bon et durable condensateur, comme l'a imaginé Mr. NICHOLSON. L'hypothése de ce savant et laborieux physicien, par laquelle il fait de chaque paire de ces pellicules, qu'il voudroit comparer à des feuilles de talc, autant de petits *électrophores* ou *condensateurs*, est, à la vérité, très-ingenieuse; c'est peut-être ce qu'on a imaginé de mieux pour l'explication des phénomènes de la torpille, en se tenant aux principes et loix connues jusqu'ici en électricité. Mais, outre que le mécanisme par lequel devroit s'opérer, pour chaque coup que ce poisson voudroit donner, la séparation respective des plateaux, de tous ou d'un grand nombre de ces électrophores ou condensateurs; devroient, dis-je, s'opérer toutes ces séparations à la fois, et s'établir, d'un côté, une communication entr'eux de tous les plateaux *électrisés en plus*, et, de l'autre côté, une communication de tous ceux *électrisés en moins*, comme le veut Mr. NICHOLSON; outre que ce

mécanisme très-compliqué paroit trop difficile, et peu naturel; outre que la supposition d'une charge électrique, originairement imprimée, et si durable, dans ces pellicules faisant l'office d'électrophores, est tout-à-fait gratuite; une telle hypothése tombe entièrement, vu que ces pellicules de l'organe de la torpille ne sont, et ne peuvent être, aucunement isolantes, ou susceptibles d'une véritable charge électrique, et moins encore capables de la retenir. Toute substance animale, tant qu'elle est fraiche, entourée d'humeurs, et plus ou moins succulente elle-même, est un assez bon conducteur: je dis plus; bien loin d'être aussi *cohibente* que les resines, ou le talc, aux feuilles duquel Mr. NICHOLSON cherche à comparer les pellicules dont il est question, il n'y a point, comme je me suis assuré, de substance animale vivante, ou fraiche, qui ne soit meilleur *déférente* que l'eau, excepté seulement la graisse, et quelques humeurs huileuses. Mais, ni ces humeurs, ni la graisse, sur-tout à demi fluide, ou fluide entièrement, comme elle se trouve dans les animaux vivants, peut recevoir une charge électrique, à la manière des lames isolantes, et la retenir; d'ailleurs, on ne trouve pas, que les pellicules et les humeurs de l'organe de la torpille soient graisseuses ou huileuses. Ainsi donc, cet organe, formé uniquement de substances conductrices, ne peut être rapporté, ni à l'électrophore ou condensateur, ni à la bouteille de Leyde, ni à une machine quelconque excitable, soit par frottement, soit par quelque autre moyen capable d'électriser des corps isolants, qu'on a toujours crus, avant mes découvertes, les seuls originairement électriques.

A quelle électricité donc, a quel instrument, doit-il être comparé, cet organe de la torpille, de l'anguille tremblante, &c.? à celui que je viens de construire, d'après le nouveau principe d'électricité que j'ai découvert il y a quelques années, et que

Fig. 1.

Fig. 2.

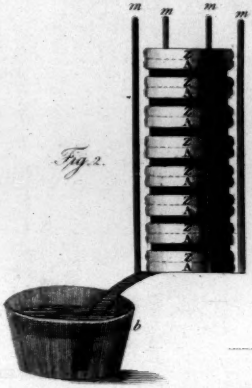


Fig. 3.

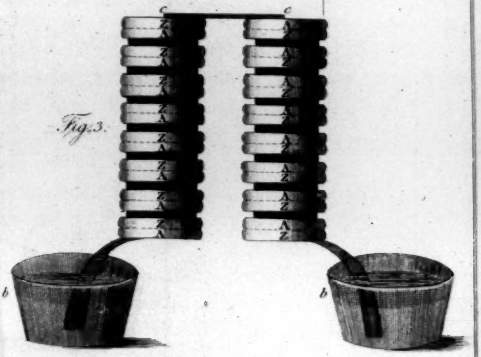
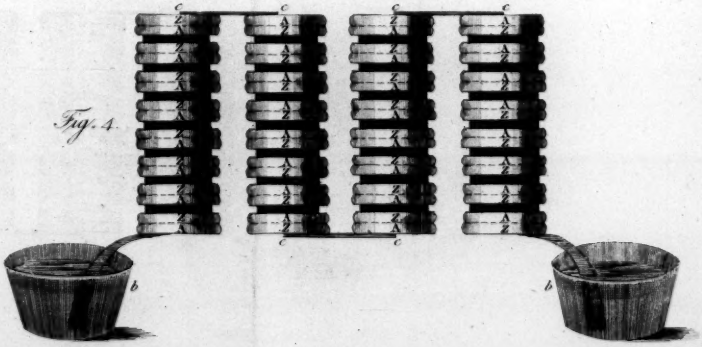


Fig. 4.



mes expériences successives, sur-tout celles qui m'occupent maintenant, ont si bien confirmé, savoir, que les conducteurs sont aussi, dans certains cas, moteurs d'électricité, dans le cas du contact mutuel de ceux de différente espèce, &c. à cet appareil, que j'ai nommé *Organe électrique artificiel*, et qui, étant dans le fond le même que l'organe naturel de la torpille, le ressemble encore pour la forme, comme j'ai déjà avancé.

XVIII. *Some Observations on the Head of the* Ornithorhynchus paradoxus. *By* Everard Home, *Esq. F. R. S.*

Read July 3, 1800.

THE specimens of this extraordinary animal which have been sent to Europe, have been deprived of the internal parts, and the skins are mostly dried, and but badly preserved. Such imperfect specimens have raised the curiosity of the naturalist, and excited the ardor of the anatomist, without satisfying their enquiries.

It was natural, under these circumstances, to reserve any observations which had been made upon this newly discovered quadruped, till the entire animal should be brought home preserved in spirit, and enable us to examine the structure of its different organs; but, finding that Professor BLUMENBACH has been led to believe that it was an animal without teeth, an opinion which must have arisen from the imperfect state of the specimen he examined, it appeared highly proper to do away the mistake, and lay before this learned Society, such observations respecting the head of this extraordinary animal, as I have been enabled to make.

My opportunities of examining the *Ornithorhynchus* were procured through Sir JOSEPH BANKS; who permitted me to have drawings made from the skin of one of a very large size, and which, from having been preserved in spirit, was more perfect than any of the dried specimens.

Any general description of the beak of this animal, which is its most conspicuous peculiarity, becomes unnecessary, as the

accompanying drawings will give a sufficiently correct idea of the outward appearances, to answer the present purpose.

It was not permitted to examine the head anatomically; but a smaller dried specimen, received from Sir JOSEPH BANKS, furnished me with the following observations.

The beak of the *Ornithorhynchus*, when it is cursorily examined, appears so strongly to resemble that of the duck, as to lead to the belief of its being calculated for exactly the same purposes; it will however be found to differ materially from it, in a variety of circumstances.

The beak is found, upon examination, not to be the animal's mouth, but a part added to the mouth, and projecting beyond it.

The cavity of the mouth is situated as in other quadrupeds, and has two grinding teeth on each side, both in the upper and lower jaw; but, instead of incisor teeth, the nasal and palate bones are continued forwards, lengthening the anterior nostrils, and forming the upper part of the beak; and the two portions of the lower jaw, instead of terminating at the symphisis, where they join, become two thin plates, and are continued forwards, forming the under portion of the beak.

This structure differs materially from the bill of the duck, and indeed from the bills of all birds, since in them, the cavities of the nostrils do not extend beyond the root of the bill; and, in their lower portions, which correspond to the under jaw of quadrupeds, the edges are hard, to answer the purpose of teeth, and the middle space is hollow, to receive the tongue. But, in this animal, the two thin plates of bone are in the centre; and the parts which surround them are composed of skin and membrane, in which a muscular structure probably is inclosed.

The teeth have no fangs which sink into the jaw, as in most quadrupeds, but are imbedded in the gum; and have only lateral

alveolar processes, from the outer and inner edges of the jaw, to secure them in their places, but no transverse ones between the two teeth.

The tongue is extremely short, not half an inch long; and the moveable portion not more than a quarter of an inch; the papillæ on its surface are long, and of a conical form. When the tongue is drawn in, it can be brought intirely into the mouth; and, when extended, can be projected about a quarter of an inch into the beak.

The organ of smell, in this animal, differs, in some particulars, from that of quadrupeds in general, as well as of birds. The external openings of this organ are placed nearly at the end of the beak, there being only the lip beyond them; while the turbinated bones are in the same relative situation to the other parts of the skull as in quadrupeds; by which means, there are two cavities the whole length of the beak, superadded to the organ of smell.

The turbinated bones in each nostril are two in number, and are distinct from each other. That next the beak is the longest, has a more variegated surface than in the duck, and has the long axis in the direction of the nostril; the posterior one is short, projects farther into the nostril, and the ridges are in a transverse direction.

The posterior nostrils do not open directly under the turbinated bones, as in the duck, but about an inch farther back, and are extremely small; the cavities of the nose, in this animal, are therefore uncommonly extensive; they reach from the end of the beak nearly to the occiput.

The beak itself is formed by the projecting bones already mentioned, covered with a smooth black skin, which extends some way beyond the bones, both in front and laterally, forming a moveable lip. This lip is so strong, that, when dried or hardened

in spirit, it seems to be rigid; but, when moistened, is very pliant, and, as has been already mentioned, has probably a muscular structure. The under portion of the beak has a lip equally broad with the upper: this has a serrated edge; but the serræ are confined to the soft part, not extending to the membrane covering the bone, and are not met with in the upper one. The extent of the lips beyond the bones, is distinctly marked in the drawings.

There is a very curious transverse fold of the external black smooth skin, by which the beak is covered, projecting all round, exactly at that part where the beak has its origin. Its apparent use seems to be to prevent the beak being pushed further into the soft mud, in which its prey may lie concealed, than up to this part, which is so broad that it must completely stop its progress.

The nerves that supply the beak, in their general course, size, and number, seem very closely to correspond with those of the bill of the duck.

The cavity of the skull bears a greater general resemblance to that of the duck than of quadrupeds: there is a very uncommon peculiarity in it, which is, that there is a bony falx of some breadth, but no bony tentorium. This is met with in no quadruped that I know of: it is found in a small degree in some birds, as the spoon-bill, and the parrot; but not at all so as to resemble the falx in this animal.

The orifice of the eye lids is uncommonly small, for the size of the animal; but the eye itself was not in a state to be examined.

The external opening of the ear was so small as not readily to be perceived: it is simply an orifice; but the meatus enlarges considerably beyond the size of the opening, and passes some way under the skin, before it reaches the organ, which in

this specimen had been destroyed. In the duck, the orifice leading to the ear is very large, when compared with the opening in this animal.

When we consider the peculiarities in the structure of the nose of this animal, which lives in water, it is natural to conclude the organ is fitted to smell in water, and the external nostrils are so placed, to enable it to discover its prey by the smell; for that purpose, the animal can apply its nose, with great ease, to the small recesses in which its prey may be concealed.

The structure of the beak is not such as enables it to take a firm hold; but, when the marginal lips are brought together, the animal will have a considerable power of suction, and in that way may draw its prey into its mouth.

EXPLANATION OF THE FIGURES.

PLATE XVIII.

Fig. 1. A view of the beak, to show the situation of the openings of the external nostrils, marked *a a*.

Fig. 2. Another view of the beak, exposing the under portion.

Fig. 3. A lateral view, to show the opening of the lips, and the situation of the eye and ear. *a*. The eye. *b*. The ear.

PLATE XIX.

Fig. 1. A view of the upper jaw and palate, to shew the teeth in their situation.

Fig. 2. A similar view of the under jaw.

Fig. 3. The bones which form the beak delineated, and the soft surrounding parts only marked in outline.

Fig. 4. A similar view of the bones forming the lower portion of the beak.

Fig. 1.

Fig. 2.

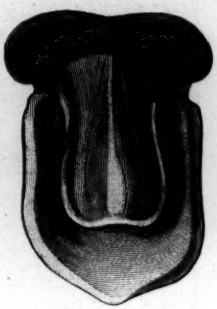


Fig. 3.

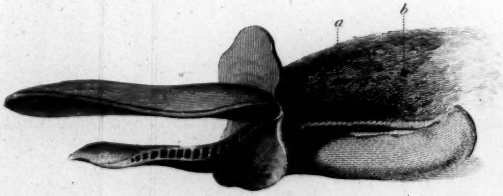


Fig. 1.

Fig. 3.

Fig. 2.

Fig. 4.

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